THE PHYSIOLOGY AND DISEASES

OF

HEVEA BRASILIENSIS.

FOMES SEMITOSTUS.

Upper surface. Specimen half dry.

THE PHYSIOLOGY & DISEASES

OF

Hevea brasiliensis

THE PREMIER PLANTATION RUBBER TREE.

BY

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THE PHYSIOLOGY & DISEASES

OF

HEVEA BRASILIENSIS.

CHAPTER I.

THE STRUCTURE OF *HEVEA*.

THE cultivation of *Hevea brasiliensis* and the systematic extraction of its latex without inflicting excessive injury on the tree, demand a more intimate knowledge of plant structure and physiology than is required in the cultivation of tea, cocoa, and other products. Therefore little apology is needed for the inclusion of an account of the structure and physiology of *Hevea* in a work which deals more especially with its pathology. Much of the information given is merely an application of general botanical principles, but experience has shown that it is just such information which is required by those who are now endeavouring to utilise *Hevea* to the best advantage.

Moreover, the information available at present is to great extent general, because the special botany of *Hevea* has not yet been worked out with any degree of completeness. It was recognised, thirty years ago, that an exact knowledge of the structure, distribution, and functions of the latex tubes of *Hevea* was an absolutely necessary foundation for its successful exploitation; and in the early eighties Dr. Trimen, who was then Director of the Royal Botanic Gardens, Ceylon, initiated the investigation of these problems by forwarding material to England, where it was examined and described by Dr. D. H. Scott and Miss A. Calvert. The results of their researches were published in the *Journal of the Linnean*

Society and the *Annals of Botany*, and up to the present day these publications contain all that is known on the subject. But the material consisted only of seedlings, none of which was more than twenty-five days old. Consequently we have exact information regarding the arrangement and formation of the latex tubes in the seedling, but very little concerning their distribution in the cortex of a tappable tree; and though the published investigations serve to establish the mode of origin of the laticiferous vessels of *Hevea*, they do not provide the particular information which the planter or inventor requires.

The Stem.

In a tappable *Hevea* stem, we may distinguish the following tissues, as we proceed from the centre outwards: the pith, the wood, the cambium, the bast, the cortex, and the outer corky layer or bark. The tissues external to the wood, which can readily be stripped off from the latter and which are usually included under the name of bark by the planter, consist of three distinct parts—the bast, the cortex, and the outer dead corky layer or true bark. When this mass is stripped off, it separates from the wood along the cambium layer.

The Pith.

The pith is of little interest to the planter. In the stem of the seedling immediately after germination, laticiferous vessels are found in the pith, and these are continuous with the laticiferous vessels in the cortex. But by the time the plants are put out in the field, these medullary vessels have been cut off from those in the cortex, and soon afterwards they dry up. Consequently no latex issues from the pith when an ordinary stem is cut across. It may further be noted that the pith of *Hevea* does not contain any bast. This is an important point when the transport of food in the plant is under consideration, since it shows that the food manufactured by the leaves cannot travel down the stem in the pith.

The Wood.

The wood consists of hollow parallel fibres running longitudinally down the stem. Some of these fibres are comparatively short and of small diameter, and taper off to a point at either end. Others are cylindrical, of large diameter, and form continuous tubes, interrupted at intervals by cross partitions. These latter are the *vessels* of the wood which convey up to the leaves the water and dissolved salts absorbed by the roots. The vessels and fibres which are formed when the tree bursts out into new foliage are larger than those formed at other times of the year, and the diameter of those formed subsequently gradually decreases until the time of leaf-fall, when they reach their minimum. Consequently the wood which is formed when the leaves are young is not so dense as that which is produced when they are old, and the density increases regularly from one extreme to the other. The change from the dense 'autumn' wood to the more open 'spring' wood is clearly evident in a cross section of the stem, and marks the limit of an 'annual ring.' In *Hevea* annual rings are produced each year after the second year; the tree does not shed its leaves in the first year. It must not, however, be thought that an 'annual ring' necessarily indicates a period of defoliation, or that the wood included in an 'annual ring' represents the growth of one year; 'annual rings' may be produced whenever the tree experiences a marked stoppage of growth. In Ceylon one ring is usually formed each year, though two may be expected if the tree is defoliated twice in the course of the year, as sometimes happens in exceptionally wet seasons; but in the Federated Malay States it is said that two 'annual rings' are formed every year, though no reason for this has been advanced, and little evidence has been adduced in support of the statement.

The wood in the centre of the stem is known as 'heart-wood,' while the exterior layers constitute the 'sap-wood.' The current of water from the roots passes up the sap-wood, the heart-wood being practically functionless. But the amount of wood which is still active varies in different species of trees. Slow-growing trees, *e.g.*, oak, have only a thin layer of sap-wood; and hence they are easily killed by ringing, because the removal of only a thin ring of wood stops the flow of water up the stem. But in trees of rapid growth, the sap-wood is much thicker and therefore they must be ringed much deeper if it is desired to kill them. In *Hevea*, the growth of the tree is rapid, and the wood is comparatively soft (one cubic foot weighs forty-one pounds); its sap-wood is consequently thick, and ordinary ringing has not so great an effect on the tree as might be expected. When a tree which has a thin layer of sap-wood is ringed by removal of the bark only, the sap-wood soon dries up or is destroyed by fungi, and the crown of the tree dies because its water supply is cut off. But when the sap-wood is thick, the inner layers may be able to provide sufficient water, until such time as the continuity of the bark has been restored.

Medullary Rays. A cross section of the stem shows numerous lines radiating through the wood. These lines are, as a rule, not continuous from the centre to the circumference. Those which start from the centre end after a short distance, while others starting at various depths reach the circumference. These are the *medullary rays.* They are thin vertical plates of more or less cubical cells, wedged in between the longitudinal fibres and vessels. In the vertical direction, they extend only for a short distance; they are not continuous from the top to the bottom of the stem. When the stem is cut longitudinally they form what is known as the 'silver grain.' These medullary rays play an important part in the transport of food in the stem, as will be explained subsequently.

The Cambium. Overlying the wood is the cambium, the most important tissue in the stem. While the cells which constitute the wood and the cortex have lost their power of multiplying, the cambium layer consists of cells which retain this power as long as the tree is alive. On its inner side it is continually adding new vessels, fibres, and medullary rays to the wood, while on the outer side it is similarly adding new elements to the bast. The increase in the diameter of the tree is therefore due to the activity of the cambium, and the only way of accelerating the growth in thickness of the wood or the cortex is to increase the activity of the cambium by supplying it with food through the normal channels. It is impossible, for example, to 'feed the cortex' and increase its thickness by supplying substances to the cortex; nor can the tree add new layers to the cortex without at the same time adding new layers to the wood.

Since the cambium is an actively growing tissue, its cells are thin walled and easily torn, and are filled with protoplasm and sap. Consequently the cortex can easily be torn off the wood, the separation occurring along the cambium layer. It is only in this way that its existence can be recognised without the aid of a microscope. In a cross section it is the line of separation between the wood and the cortex; and it comes close to Euclid's definition of a line—length (round the wood) without breadth. Its thickness, in *Hevea*, varies from twenty-five to forty microns, *i.e.*, from one-thousandth to one-six-hundredth of an inch. One is often asked by the planter, in his desire for something tangible, to cut out a piece of this much-discussed tissue so that he may really understand what it is like. The details given here will serve to show why it is impossible to comply with this request; one cannot, with an ordinary knife, cut off a piece of cambium without at the same time including some of the wood and the cortex, owing to the extreme thinness of the cambium layer.

When the cortex is torn off the wood the cambium is left as a thin, semi-fluid, slimy layer on both surfaces. For this reason the cambium was regarded as a liquid when botanists first began to inquire into the internal structure of plants; and Dr. Carpenter, in his *Vegetable Physiology*, published about the middle of the last century, calls it a glutinous fluid.

It will be understood from the above remarks that it is idle to talk of 'touching the cambium but not penetrating it' with any tapping instrument whatever. If the knife or pricker touches the cambium it must penetrate it.

The Cortex.

The tissue which lies between the cambium and the outer layer of corky bark includes both the bast and the cortex, but it may be more conveniently dealt with under the latter name. It is the whole of the living tissue external to the cambium, and is of supreme interest to the planter, as it is the region from which all the latex is obtained. On trees twenty to thirty years old the cortex is about five or six millimetres thick (one-fifth to one-quarter of an inch), and this would seem to be its maximum thickness. Parkin quotes Ridley to the effect that the average thickness of the trunk bark of *Hevea*, eleven years old, at Singapore is 9·5 millimetres (three-eighths of an inch), and he adds that that is about the average for the Peradeniya trees, but it would appear that this measurement includes the outer dead layer—the true bark—as well as the cortex. In a cortex five millimetres thick the latex flows chiefly from the inner two millimetres; the middle millimetre yields a little, but the outer two millimetres yields only a few minute drops or nothing. Practically rather less than half the cortex will yield latex when cut.

The thickness of the cortex differs in trees grown at different elevations in Ceylon, even though they are of the same girth. It is generally stated that the cortex of trees grown at an elevation of 1000 to 1500 feet is thinner than that of trees grown at lower elevations. But no exact measurements have yet been published.

The bast, which lies next to the cambium on the inner side of the cortex, is built up of fibres and vessels like the wood. In this case the vessels are known as '*sieve tubes*,' since the cross partitions in them are perforated by numerous openings. These vessels differ therefore from those of the wood, in that the cross partitions of the latter prevent the passage of any substance up the tube except such as are dissolved in the sap, whereas the perforated partitions of the 'sieve tubes' allow the free movement of insoluble jelly-like substances (colloids). The 'sieve tubes' are the channels by which all the food manufactured by the leaves passes down the stem.

The medullary rays, which are so evident in the wood, are continued into the bast, but in the latter situation they are not distinguishable without a microscope.

The internal colour of the cortex of *Hevea* varies in different trees, and apparently this variation is not dependent on age. In some trees it is white or faintly yellowish, in others, it is a clear pink or reddish tint, while in others it is mottled, red and white. It is important that the planter should familiarise himself with the various tints and appearances of healthy *Hevea* bark in order to be able to distinguish the abnormal discolorations due to disease. Healthy cortex is often a clear pink or red internally; this, once seen, cannot be confused with the dirty red or claret colour which characterises *Hevea* 'canker.'

The cortex of *Hevea* contains numerous minute clusters of hard, thick-walled cells which are known as 'stone cells.' They are similar in structure to the groups of cells found in the flesh of the pear to which the 'gritty' feel of the latter is due. Some *Hevea* cortices contain more stone cells than others. They appear to be more numerous in old trees than in young trees, and in some cases they are more numerous in the renewed bark than in the original bark. It is generally stated that it is impossible to put so many cuts to the inch on renewed bark as on the original bark. This may be due to the greater friability of the renewed bark because of the greater number of stone cells in it, but investigations into this point have not yet been completed. Sometimes the stone cells form a brown powdery layer between the cortex and the outer dead bark.

In the mature stem the laticiferous vessels occur only in the cortex. Consideration of these is deferred to a later section.

The True Bark. The outer dead corky tissue, the true bark, whether on the untapped stem or on the tapped surface, is not produced merely by the drying up of the outer tissues of the cortex. It is the result of a definite vital process, similar in many respects to the formation of new wood and bark by the cambium. It is the product of another cambium, the cork cambium or *phellogen*, which lies between the dead corky bark and the living cortex. When the young stem is still green this cambium is developed at a slight depth beneath its epidermis, and proceeds to form a layer of cork cells on its outside all round the stem. The tissues outside this ring of cork are thus cut off from their water and food supply, and consequently they dry up and turn brown. On its inner side this cork cambium builds a green layer known as the *phelloderm;* this is seen when the dead bark is scaled away from the underlying cortex, though the green colour may sometimes be wanting. The green is not developed when the bark forms irregular scales, nor is it present at first on the renewed cortex, but it reappears in the latter situation when the renewed bark is old.

In one important particular the cork cambium differs from the stem cambium, *i.e.*, from that which produces the wood and bast. The latter might be described as a permanent tissue, in the sense that the same layer of cambium persists throughout the life of the tree, continually throwing off new wood and cortex on either side. But the layer of cork cambium does not persist. After one cork cambium has been formed and has cut off the outer tissues by a layer of cork, another cork cambium is formed, deeper in the cortex, which similarly cuts off all the tissues external to it. Thus the bark is not the product of one persistent cork cambium, but of a succession of cork cambiums produced at continually increasing depths in the cortex.

It will be understood from the preceding details that while the main cambium of the stem is continually adding new layers to the inner side of the cortex, the successive cork cambiums are at the same time cutting off the outer layers and converting them into dead corky bark. Hence the cortex does not increase indefinitely in thickness. It would appear that the maximum thickness of the cortex is about five or six millimetres, and that when that thickness has been attained, the processes of addition and subtraction balance one another.

On young stems, up to two or three years old, the outer dead bark forms a continuous layer, usually grey or whitish. As the stem grows older the bark becomes brown and cracked. As a rule, both kinds of bark may be seen on the same tree—the continuous greyish bark on the branches and upper part of the stem, and the brown rough bark on the lower. But on some trees the whole of the bark is smooth and grey, even when they have reached a tappable size; and hence there has arisen the idea that there are two varieties of *Hevea brasiliensis*, one of which has rough bark, while the other has smooth bark. Opinions as to the relative value of the two types of tree differ. It is generally stated that the cortex of the smooth-barked tree is thinner than the normal, and this is what might be expected since the smooth bark indicates the persistence of a younger stage or condition. But while some planters declare that it is less laticiferous, others claim that it yields more latex than the rough-barked cortex. Evidently no reliable conclusion can at present be drawn on this point. It may be noted that the smooth-barked tree has been referred to as a tree with white bark, or, in one case, as a tree with pink bark. These colours do not refer, as might be thought, to the internal colour of the cortex, but to the external colour of the dead bark, and in neither case is the colour a character of the tree. On the contrary, these colours are due to the growth of lichens or algæ on the smooth bark, where they secure a better footing than on rough bark. In general, the stems are covered by an extremely thin white lichen which adheres so closely to the bark that it seems to be part of it; but in some cases they are covered by a pink or reddish alga similar to that which is common on cocoanut stems. It is not known why certain trees retain this smooth-barked condition. As they occur indiscriminately among rough-barked trees, their condition cannot be attributed to the lack of food, unsuitable soil, or other 'environmental causes.'

Scaly Bark. It has already been stated that the dead bark, in its earlier stages, is smooth, and that it becomes cracked and rough later, this change being due in part to the expansion of the stem. In both these stages, the cork cambium apparently forms a continuous layer of cork all round the stem at the same time. But under certain conditions the character of the cork cambium apparently changes, and instead of cutting off a continuous layer all round the stem, it forms isolated patches, each of which cuts off a separate piece of bark. Consequently the bark becomes scaly. The scales thus produced vary in size, and may attain a length of twelve inches and a breadth of about six inches; they are quite loose, and can easily be detached from the stem, leaving as a rule only a thin brown layer overlying the cortex. They are apparently a normal feature of old *Hevea* trees, but they have in some cases caused considerable alarm by appearing on young trees. In such cases the scales are usually confined to definite patches, and though more than one patch may occur on the same stem, it is rare that the whole of the stem becomes scaly. Under such circumstances, these localised scaly patches have naturally been assumed to be due to some disease, generally to *Hevea* 'canker,' but there is no reason to believe that it is other than a normal phenomenon in *Hevea*, and as will be seen later, 'cankered' *Hevea* bark, *i.e.*, bark attacked by *Phytophthora Faberi*, is not scaly. What is not at present understood is why this phenomenon, even if it is a natural feature of old trees, should appear on young trees, and why it should be confined to one part of the stem instead of affecting the whole of the bark at the same time.

All tapping operations appear to hasten the production of scaly bark. Indeed, it may be said that all renewed cortex if left long enough becomes scaly. A similar production of scales usually occurs whenever a tapped surface is abandoned before all the original cortex has been excised; the tapping cut is, of course, left with a rectangular edge, but in course of time a cork cambium is formed which cuts off this projecting edge in a series of scales, so that the change from the tapped to the untapped cortex is no longer abrupt. In one case, in which a breadth of two or three inches of untapped cortex had been left between two consecutive tapping surfaces, the whole of this untapped cortex subsequently scaled off down to the level of the renewing cortex on both sides of it.

It may be noted that the bark of the common Jak tree (*Artocarpus integrifolia*), which is also laticiferous, though its latex does not contain rubber, behaves in the same way as that of *Hevea*. The bark of a young tree is smooth; that of the old tree is scaly, and the scales are easily detachable.

Primary Growth.

The foregoing account of the structure of the stem of *Hevea* would probably have been sufficient, or more than sufficient, to satisfy the needs of the planter, and to explain the various technical terms which he is likely to meet with in the literature of the subject, were it not that reference has already been made, in several publications, to other stages in its history in order to support theories which have been put forward in explanation of other phenomena. The stage already described—that which alone is of interest to the planter—is the stage of *secondary growth*. In its *primary stage*, the structure of the stem is quite different. Instead of a complete cylinder of wood, surrounded by cambium and bast, it then contains a number of separate strands, the *vascular bundles*, running longitudinally through it. These vascular bundles are embedded in thin-walled ground tissue, and in a cross section of the stem their cut ends are seen arranged in a ring. Each bundle consists of three longitudinal strands, the wood on the inner side, the bast on the outer, and the cambium between them. In this stage the stem is green, and covered by a continuous layer of cells known as the epidermis.

In *Hevea*, the primary stage is of very short duration. Part of the ground tissue which lies between the vascular bundles is soon converted into cambium, in continuation of the cambium in the bundles, so that a complete cylinder of cambium is formed. The cambium then produces wood on its inner side and bast on its outer side as already described. The stem has now entered upon its secondary stage, so far as its wood is concerned, and this second mode of development of the wood and bast persists throughout the life of the tree. In the centre is the pith, into which project the strands of wood of the original vascular bundles. This is surrounded by the wood, which is traversed radially by the medullary rays. Next to this lies the thin layer of cambium; and outside the latter the bast into which the medullary rays penetrate.

It will be understood from the above details that secondary growth has begun whenever the stem possesses a complete cylinder of wood. Thus the green shoots of *Hevea* are in the secondary stage as far as their wood is concerned, though the cortex is still in its primary stage. In order to see the separate vascular bundles in *Hevea*, one must examine the seedling as soon as it has germinated, or its tip when it is only a few days old.

The secondary cortex develops later than the secondary wood, the change being indicated by the loss of green colour of the shoot and the acquisition of a grey, harder outer coat. Thus all the green shoots of the tree have primary cortex, though their wood may all be in the secondary stage. When nursery plants are stumped and planted out, all that is alive is of secondary growth.

'Primary' and 'secondary' are well-established botanical terms, and the botanists who have written about *Hevea* have always employed them in their accepted sense. Parkin, for example, in his circular on caoutchouc (June 1899) wrote: 'After the primary growth has ceased the stem begins to grow in a definite manner in thickness only.' But unfortunately they have been misapplied in recent years, 'primary cortex' having been used to mean the cortex first tapped (*i.e.*, what is usually called 'original bark'), and 'secondary cortex' to mean the first renewed cortex. This double use of the terms can only result in confusion; and we already have A declaring that B is wrong, when as a matter of fact each is using the same term but with a different meaning. When a botanist states that the rubber formed during primary growth is weaker than that produced in secondary growth, the planter interprets the statement as meaning that the rubber in the original cortex is weaker than that in the renewed cortex. The renewed cortex should be designated 'first renewed,' 'second renewed,' &c., not 'secondary cortex,' 'tertiary cortex,' &c.

The Root. The structure of the root of *Hevea* need not detain us. It may be noted that the cortex of the root is usually a clear red internally, and that it is covered by a thin papery layer which is white and smooth on the exterior. Both these points are of use when examining roots for possible diseases.

Hevea brasiliensis never has a compact root system. It has a well-developed tap root and far-spreading lateral roots, the length of the latter depending on the nature of the soil, the available moisture, and other causes. Some years ago it was declared that the lateral roots of *Hevea* grew in length at the rate of one foot per year, but it is now admitted that this was a decided underestimate. A radial spread of twenty to thirty feet in the first ten years, in ordinary estate soil, is not uncommon, and in exceptional cases two-year-old trees have developed lateral roots more than twenty feet in length. In the *Times* of Ceylon, August 28, 1907, it was recorded that one nine-year-old tree, grown in low swamp land, possessed a tap root only five feet long, but a lateral spread of the surface roots to a distance of seventy-two feet.

The general rule would appear to be that the growth of lateral roots is small during the first, or the first two years, but very rapid from the third to the sixth year. In later years their extension depends largely upon the distance at which the trees are planted and the style of tapping to which they have been subjected, since both these factors influence the quantity of food which is available for the construction of new roots. When *Hevea* is planted fifteen feet apart, their roots will, in general, have met by the end of the fourth year.

The various rules for estimating the length of lateral roots, which are quoted from time to time, have none of them any valid basis. Among these the chief are (*a*) that the length of the lateral root is always half the height of the tree, and (*b*) that the tips of the roots lie vertically below the ends of the branches. The latter view belongs to popular natural history, and provides material for rhapsodies on the wonderful correlation between the various parts of the tree; but it has no foundation in fact. In most tropical trees it will be found that the ends of the lateral roots are far beyond the shelter of the branches. The spread of lateral roots is governed chiefly by the distribution of moisture in the soil, and not, as is generally supposed, by the presence of an excess of plant food in any particular direction. Roots grow towards a moister region, and they often attain an enormous length in the direction of any neighbouring pond or river. The presence of large numbers of tea roots in the holes containing buried prunings is due to the fact that the decaying prunings retain moisture, not because there is an excess of food there.

Though roots grow towards a source of water, they will not, except in certain well-known cases, live permanently under water. Like all the other parts of the plant, they require a supply of oxygen, and they are suffocated if they are deprived of it by permanent immersion in stagnant water or in swampy soil. When *Hevea* is grown in swampy soil, where the water table lies near the surface, the tap root does not grow below the water level, or, if it does, it soon decays. In some cases such trees have to be propped up to prevent them falling over, but in general their lateral roots afford them sufficient support, provided that they are not exposed to strong winds.

As the roots will not grow into the unaerated lower layers of such soils, the upper layers become dense masses of feeding roots, so much so that one is at times walking on a spongy carpet of fine white rootlets. It has been picturesquely stated that in such situations the soil is so poor that the roots come up to the surface 'appealing for food'; but it is the need of oxygen, not the need of food, which confines them to the surface layers of the soil. Decaying logs lying in such areas are soon permeated by *Hevea* roots, because the spongy, decaying wood retains moisture, and thus favours their development in that direction; their occurrence in such a situation does not indicate 'the demand of *Hevea* for humus.'

The absorption of water and nutrient inorganic salts from the soil is effected only by the finest white rootlets, and in these only by a short region near the tip. The older roots are functionless in this respect.

Laticiferous Vessels.

Plants which yield latex are furnished with special sacs, tubes, or vessels in which the latex is stored. In general these tubes form a connected system, extending, more or less longitudinally, through all parts of the plant—the roots, stem, leaves, fruit, &c. The laticiferous system does not replace any of the ordinary plant tissues already described; it is an additional development which exists in a comparatively few plants, and those which have acquired it would, to all appearances, be quite complete without it. There are two methods of formation of latex tubes: one, the commoner type, is found in *Ficus, Castilloa*, &c.; while the other occurs in *Hevea* and *Manihot.*

In the former type, the laticiferous tubes present in the full-grown parts send out branches into the growing tissues. Thus the new tubes in the youngest parts of the plant are direct prolongations of those in the older parts, and are not cut off from them by any cross partitions in the tube. In some cases the laticiferous system established in the seedling continues to branch and grow on into the newly-formed parts, so that the whole system forms one continuous branched tube from the beginning.

In *Hevea* and *Manihot* the laticiferous vessels are formed from rows of special cells, which are arranged more or less longitudinally in the cortex. These cells are specially laid down by the cambium as latex cells, and may be distinguished from the neighbouring cells by their greater length and also by their contents. The cross walls which separate the cells are absorbed, and in this way a continuous tube is produced. At the same time the lateral walls between two vertical rows of cells are also absorbed, so that the tubes communicate with one another laterally. This formation by the fusion of a series of cells one above the other produces an articulated vessel, quite distinct in structure from the continuous tubes of *Castilloa.*

The absorption of the walls which separate the cells was described by Scott from an examination of the embryo of *Hevea brasiliensis*, *i.e.*, the young plant still contained in the seed, at the commencement of germination. The laticiferous tubes then occur only on the bast side of the separate vascular bundles; but they are even then well developed, and form a complex network which contains abundant latex. Numerous and extensive perforations occur in the lateral walls. The process of absorption was best seen in the cotyledons (*i.e.*, the two leaves within the seed), where, at this early stage, it is not complete, in spite of the advanced differentiation of the laticiferous tissue. In some places only a ring of cellulose remained within the tube to mark the former position of the cross wall; in other places only half the wall had been absorbed; while in others the wall was reduced in thickness, but was not yet perforated. It appears to be a very general rule in the development of these vessels that the perforation of the lateral walls begins at an earlier stage than that of the transverse walls. The 'curious rod-like bodies,' figured by Wright in his representation of a latex tube of *Hevea*, are remnants of the lateral walls.

In the seedling, absorption of the cross walls takes place during the first stages of germination, and sections through any region in which latex tubes occur show that the latter form a complex intercommunicating network. In older seedlings latex vessels occur in the pith, and are connected with the vessels of the cortex at the nodes, *i.e.*, at the places where the leaves are borne on the stem. As the stem increases in thickness by secondary growth, these inner latex vessels are cut off from those in the cortex by the cylinder of secondary wood, and, consequently, they dry up.

In the secondary cortex, *i.e.*, the cortex from which the latex is extracted by the planter, the laticiferous vessels run more or less longitudinally down the stem. They are not the result of any haphazard transformation of ordinary cells of the cortex; but, like the vessels of the wood and the sieve tubes in the bast, they are formed by rows of cells laid down by the cambium specially for the production of latex tubes. In a non-laticiferous tree the cambium makes, on its outer side, cells which, ultimately, become sieve tubes, bast fibres, medullary rays, &c.; in *Hevea* it makes, in addition, rows of cells which ultimately become latex vessels It follows from this that it is impossible to cause any increase in the number, or any new formation, of latex tubes in a piece of mature cortex; more latex tubes can only be obtained by the addition of new layers to the inner surface of the cortex through the agency of the cambium.

It has been stated that in Ceylon the latex vessels are nearer to the cambium than in the Federated Malay States, and that, consequently, the cortex must be cut more deeply in the former country to obtain the best yield. This theory is impossible, and of course it has not been supported by any anatomical evidence.

As already stated, the cambium lays down special cells, which are destined to become part of the laticiferous system These cells are arranged in more or less vertical rows, and are larger than the neighbouring cortical cells. They are distinguished, too, by their contents, which, if not exactly latex at first, are soon converted into latex. Soon after their formation their cross walls are absorbed, so that a continuous tube is produced shortly after the cells have been severed from the cambium layer. Where two tubes lie side by side their lateral walls also become perforated, and thus the freest communication is established between them. The perforations in *Hevea* are more extensive than in *Manihot*.

It has been argued that the presence of laticiferous vessels in all stages of decomposition (*i.e.*, with their cross walls in some places completely absorbed, and in others only partly absorbed or still intact) may account for variations in the yield of a tree, and the same fact has been invoked to account for 'wound response.' According to this theory, a tree may yield only a small quantity of latex because its laticiferous vessels are still blocked by cross partitions which prevent a free flow, and the yield increases when these cross walls are absorbed. But, according to our present knowledge, the cross walls are absorbed shortly after the cells have been formed, and therefore the incomplete vessels must be confined to a very thin layer overlying the cambium. After the cortex has attained its maximum thickness, the thickness of this incomplete zone should remain practically constant. It does not seem probable that the liberation of the latex in this thin layer can materially affect the quantities obtainable in two successive tappings. Moreover, there is no reason to suppose that the act of tapping stimulates the absorption of the cross walls in other parts of the tree. In order that the presence or absence of cross partitions in the laticiferous system should affect the yield, it would surely be necessary to prove that their absorption is a periodic phenomenon, *i.e.*, one which occurs only at certain definite intervals, and the variation would then in all probability be seasonal. It is impossible to imagine that the relative frequency of cross walls in the latex tubes can explain the phenomenon of 'wound response.'

Mention has already been made of the medullary rays. It may be recalled that these are thin vertical plates of tissue which run from the cortex radially into the wood. When two latex tubes which run side by side down the stem meet the edge of a medullary ray, they separate and curve round it, one on either side, uniting again below. Therefore in a tangential section (*i.e.*, one which shaves the cortex parallel to the stem) the medullary ray appears as a narrow-oval group of cells bordered on either side by a latex tube, while in a radial section the latex tubes are seen to lie vertically side by side in a band overlying the medullary ray. The general direction of the latex vessels is therefore governed by the direction of the medullary rays.

In the leaf, the latex vessels accompany the veins to their ultimate ramifications, and this leaf system is continuous with that in the stem. There is therefore nothing inherently improbable in the view that latex is being continuously formed in the leaf, except for the objection that no one has yet demonstrated that latex tubes are able to transport manufactured material. But if the theory that the latex tubes are refilled by the activity of the tubes themselves is correct, a continuous formation of latex in the leaf would be expected.

The experiment described below, which is due to Mr. C. Northway, illustrates to some extent the fact that the latex vessels are continuous. With a sharp penknife, half a dozen horizontal stabs in a vertical line and about an inch apart are made through the cortex of a *Hevea* as quickly as possible. It is found that latex drops from the uppermost and the lowest cuts before it drops from any of the intervening four.

Results obtained in tapping indicate an extensive connection of the latex tubes. Lock and Bamber, tapping ten trees daily, obtained 1971 grams of rubber in the first ten days' tapping, and 706 grams in the 110th-120th days' tapping; operations were then transferred to the other side of the tree, which yielded 1185 grams in the first ten days' tapping, and 259 grams in the 170th-180th days' tapping, *i.e.*, the last ten on the second side. The percentage of rubber in the latex of the first ten tappings on the first side was 40·4; in the first ten tappings on the second side it was 30·4. The results in two-day tapping were exactly similar; the first ten tappings on one side yielded 1537 grams, and the first ten on the other 765 grams, while the percentages of rubber in the latex were 40·7 and 30·1 respectively. In both cases, when tapping was transferred to the other side of the tree, less latex and that of poorer quality was obtained.

As far as our present knowledge indicates, the laticiferous system of *Hevea* forms a network extending from the leaves to the roots, and consists of a continuous tube, except in those parts which have just been added to it by the cambium.

CHAPTER II.

LATEX AND RUBBER.

THE latex of *Hevea* is a feebly alkaline liquid in which the globules of caoutchouc are suspended; it contains proteids in solution, as well as small quantities of resins, sugars, and mineral salts. In colour it is white or cream-coloured, or sometimes deep yellow. Parkin states that the cream-coloured (yellowish) latex is constant for certain trees, and that of thirty-two trees examined at Peradeniya, nine have a decidedly cream-coloured latex. None of these have, however, a deep yellow latex. Ridley states that the increase in the quantity of the latex which occurs after a few days' tapping is accompanied by a change in the colour of the *coagulated rubber* from a tint of yellow to white, but he does not record the colour of the latex. A similar change has been recorded for South India, where it is said that the freshly coagulated rubber is yellow in the dry season and white in the wet season. The cause of this change is not known. It has been noted that when a stem is thickly covered with burrs, its latex is frequently deep yellow.

There is no starch in *Hevea* latex, either from trees thirty years old or seedlings three days old. In the latter case the other tissues of the stem are completely filled with starch, but if the stem is cut across and the latex allowed to drop from the cut surface, no starch grains can be found in the drop.

Seeligman states that the globules of caoutchouc in *Hevea* latex have a mean diameter of 3·51 microns. Parkin states that they are almost too small to be measured, and suggests that Seeligman has given the measurement of *Castilloa* globules instead of those of *Hevea.* Victor Henri states that their diameter is 1 micron. Examination of the latex from a green shoot from a twenty-three-year old tree at Peradeniya showed that the latex contained a large number of very minute granules, too small to be measured with a magnification of 600 diameters, and a few globules about 1 micron in diameter. But the latex drawn in the ordinary way from the stem of the same tree contained globules which varied in diameter from 0·5 to three microns. Many of these globules were furnished with a distinct 'tail'; in one instance a globule three microns in diameter possessed a tail five microns long and 0·5 micron wide. It would appear from this that Parkin's statement refers to the latex of young stems or green shoots. It may be noted that the caoutchouc globules in the latex of the *Castilloa* which is supposed at Peradeniya to be *Castilloa elastica*, measure from 1·5 to three microns in diameter. Therefore the largest *Castilloa* globules are not larger than the largest *Hevea* globules, but the average diameter of the former is greater.

As it has frequently been suggested that the caoutchouc in the latex takes some time to mature, and that immaturity is denoted by the smaller diameter of the globules, an attempt was made to obtain latex from the older parts of the cortex only. The cortex was scraped away until minute drops of latex exuded, and these were at once transferred to a glass slide by means of a brush. It was found that the diameter of the globules varied from 0·5 to three microns, as before, but most of them were small, and there was a large proportion with 'tails.' The average diameter of the globules in the outer older cortex was less than that of the globules in the whole cortex. The tree examined in this instance was twenty-three years old, and had never been tapped. In all cases the latex was kept fluid by the addition of a dilute solution of ammonia.

The latex of *Hevea* coagulates when it becomes acid, either by the actual addition of an acid or by the development of acids through the action of bacteria, &c. According to Parkin, coagulation is brought about by the separating out of the proteid matter from solution, which entangles in its meshes the rubber particles so as to form a clot, and he gives the following explanation of the behaviour of *Hevea* latex toward acids. 'The latex is slightly alkaline. The proteid is of such a nature as to be insoluble in neutral solution, but soluble in alkaline or acid media. A small quantity of acid is necessary to neutralise the alkalinity, and this precipitates the proteid in a flocculent manner, collecting together the caoutchouc particles. If too much acid is added, then the proteid remains still in solution, being now in an acid medium.' Complete coagulation is shown by the absence of any turbidity in the remaining liquid.

The amounts of various acids required to effect complete coagulation of *Hevea* latex were determined by Parkin. He found that sulphuric acid was the most effective, but recommended acetic acid, since with the latter the range for complete coagulation is greater than with any other. Thus with sulphuric acid, coagulation was not complete with 0·05 per cent. of acid, about complete with between 0·1 and 0·2 per cent., but not complete with 0·25 per cent. On the other hand, with acetic acid coagulation is complete with between 0·09 and 0·39 per cent. and almost complete between 0·025 and 0·8 per cent. Therefore, with acetic acid, the acid may be added in quantities either four times below the proper amount or nine times above it with very little waste of rubber, whereas, with the other acids experimented with, such variation would entail a very incomplete coagulation. Parkin recommended the use of three volumes of ordinary acetic acid (B.P.) to 100 volumes of pure latex. This is equivalent to one volume of glacial acetic acid to 100 volumes of pure latex. Biffen had previously found that the smoke of the burning palm nuts, which is used to coagulate the latex in the Amazon valley, contained acetic acid and creosote. Parkin's recommendation agrees therefore in general outline with the native practice.

Hydrofluoric acid has been strongly recommended as a coagulant, but no experiments similar to those with acetic acid quoted above have been recorded, and therefore the range within which it effects complete coagulation is not known. It is claimed that hydrofluoric acid is a disinfectant and 'arrests decomposition,' whereas in acetic acid there is nothing to 'arrest decomposition,' and therefore the rubber coagulated by the latter becomes mouldy and tacky. But it has not yet been proved that tackiness is dependent upon any 'decomposition,' and certainly coagulation with hydrofluoric acid does not prevent the growth of moulds on the rubber.

At the Rubber Exhibition held at Peradeniya in 1906, a medal was awarded to Mr. W. J. A. Bird for methods of coagulating latex by the use of cream of tartar, but this coagulant has not been generally adopted in spite of the fact that Mr. Bird obtained the gold medals for the best *Hevea* biscuits, both in the open and Ceylon classes. Two recipes were given:—

(*a*) One dram cream of tartar, dissolved in one ounce of cold water, added to a pan of latex of about forty-eight ounces.

(*b*) Half a dram of cream of tartar, dissolved in four ounces of fresh rubber whey, added to a pan of latex of about forty-eight ounces.

More recently a similar coagulant, under the name of Coaguline, has been put upon the market by a Deli planter. It is said to consist of tartar emetic, three per cent.; formaldehyde in the form of formalin, 0·5; carbolic acid, 0·5; water, 96 per cent. The effective coagulant in the mixture is the tartar emetic; the formaldehyde prevents coagulation, but may act as an antiseptic, as does the carbolic acid. Parkin experimented with carbolic acid as an antiseptic, but rejected it in favour of creosote, because the latter does not evaporate so quickly.

Spence has recently shown that if the same sample of latex is divided into two parts, and one of these is coagulated by sulphuric acid, while the other is coagulated by other methods, the former is 'tacky,' though the latter is sound. This provides an additional reason against the use of sulphuric acid, even though it is cheaper than the other coagulants.

The addition of ammonia or formalin to *Hevea* latex prevents coagulation. It is said that the ammonia neutralises the acids as they are formed and thus keeps the latex alkaline, while the formalin stops putrefaction and thereby prevents the development of acidity. In either case precipitation of the proteids is prevented. Parkin recommended a solution of one volume of ordinary ammonia in 100 volumes of water, and apparently added about an equal quantity of this to the pure latex. With formalin, one volume of commercial formalin to 400 volumes of latex will prevent the development of bacteria. As a rule no measured quantities are added to latex which it is desired to preserve, but the ammonia or formalin is added until the liquid smells strongly of it. Exact experiments on this point have apparently not been carried out. The latex must be kept in a closed vessel after the addition of either preservative, to prevent the evaporation of the latter and consequent coagulation.

Some misapprehension exists as to the action of formalin on *Hevea* latex. It has been stated that formalin removes the proteids from latex, and therefore prevents decomposition of the rubber. 'Formalin eliminates the objectionable albuminous material by preventing the coagulation with the caoutchouc, and if it be thought expedient, elimination can be made more secure by further adding to the latex, after the addition of formalin, some substance in solution capable of precipating the protein, which at the same time cannot harm the caoutchouc. The most satisfactory substance to use for this purpose is a neutral sodium sulphate.' This quotation appears to be a misinterpretation of Weber's suggested treatment for reducing the amount of proteid in *Castilloa* rubber. In that case the formalin merely acts as an antiseptic and prevents coagulation; and on adding the solution of sodium sulphate the rubber rises to the surface as a creamy mass, leaving the proteids in solution below. This treatment has been tried with *Hevea* latex, but without any success, since the latex does not 'cream.'

Parkin has stated the rubber prepared from *Hevea* latex does not blacken, and that any development of a dark colour is due to the growth of moulds on it. He suggested that the darkening was due to the penetration of dark-coloured fungus threads into the damp rubber. 'Para rubber then need not be of the usual dark colour; this is a defect, and should be prevented.' In this he was probably misled through preparing his samples of rubber by boiling the latex before adding the acid, whereby he obtained a translucent biscuit, 'about the colour of gelatine.' It is well known that the rubber which is allowed to coagulate and remain on the tree always becomes black, and this is quite independent of any growth of bacteria or moulds upon it. Certain bacteria do produce a black discoloration in rubber, but such discoloration occurs in isolated patches, and the granules to which the colour is due are readily discernible under the microscope; this is quite different from the general darkening which *Hevea* rubber undergoes when prepared by the ordinary cold process. As far as moulds are concerned, those which are known to grow on prepared rubber have hyaline threads, and these do not penetrate to any appreciable depth; I have never found dark-coloured fungus threads in *Hevea* rubber.

Parkin's error is the more surprising since he states in the same paper that the darkening of *Castilloa* latex is caused by an enzyme, and that if the freshly-drawn latex is boiled the discoloration is prevented, because the enzyme is destroyed by boiling. The darkening of *Hevea* rubber is due to exactly the same cause, and, as Parkin himself showed, it does not occur if the latex is heated so as to destroy the enzyme. Parkin's method of heating the latex when preparing *Hevea* rubber has recently been revived, and is now in use on a large scale for the production of clear amber-coloured biscuits to meet the present demand of the rubber market. The latex, diluted with water if necessary, is heated to a temperature of 160° F. for ten minutes, preferably by immersing the vessels containing it in hot water, or the wet biscuits are immersed in hot water. The latter method is not satisfactory unless each biscuit is immersed separately.

It has been observed that in wet weather the coagulated latex often begins to blacken much sooner than at other times, the 'scrap' becoming black on the surface within a few hours.

Rubber from old trees is generally darker than rubber from young trees. Ridley states that as a general proposition coloured rubber is the strongest, but he apparently refers to the colour of the freshly-coagulated rubber, not of the dry biscuit; on this supposition the white wet rubber, which he says is obtained in the wet season, is weaker than the yellow wet rubber obtained in the dry season *from the same trees.* Whether there is any difference in strength between pale rubber prepared by the hot method and dark rubber prepared in the cold from the same latex has not yet been ascertained.

It is often stated that there is no rubber in *Hevea* latex when it is in the latex tubes, but that it contains some substance which changes to rubber on exposure to the air. This idea is arrived at from a comparison of *Hevea* with *Ficus elastica*, *Landolphia*, and other rubber-yielding plants. If a piece of the bark (cortex) from a dead stem of *Ficus elastica* is broken carefully, strands of rubber are seen stretching from one piece to the other. But if the same experiment is tried with a piece of *Hevea* cortex, similarly from a dead stem, strands of rubber do not occur. Hence it is argued that there cannot be any rubber, as such, in *Hevea* cortex. But against this we have the fact that if the cortex is cut from the tree and immediately placed in alcohol, rubber can be seen in the latex tubes when sections are cut and examined under the microscope; this does not necessarily prove that rubber was present in the living cortex, but it certainly excludes any possibility that exposure to air is necessary for its formation. Moreover, rubber does exist in the dry *Hevea* cortex just as it does in the dry *Ficus* cortex, and it can be extracted by the usual solvents. Further, if the experiment is tried with *Hevea* cortex which has been killed by 'canker,' strands of rubber are produced just as with *Ficus*. Cankered cortex is softer than normal cortex; and the explanation of the non-occurrence of strands of rubber on breaking normal *Hevea* cortex would appear to be simply that the latter is more brittle than the other cortices and breaks with a sudden fracture, thus rupturing the fine strands of rubber.

Wound Response.

When *Hevea brasiliensis* is tapped for the first time, or after a long period of rest, the yield of latex from the first tapping is less than that obtained from the second tapping, and the quantity may continue to increase each time until about the sixth tapping. This phenomenon appears to be well known to the natives of the Amazon valley, who say that a tree has not got used to tapping when it does not yield a normal quantity of latex when freshly tapped. In the Federated Malay States the first tappings were said to 'call the latex.' This peculiarity has in some cases given rise to serious misunderstanding as to the value of rubber plantations, and they have been condemned as unprofitable because only a small yield was obtained at the first tapping. In one instance some amusing 'facts' were put forward to explain why the trees, which were growing on good soil in a favourable situation, yielded scarcely any latex at the first tapping. It was asserted that latex was a reserve food, and as the trees were growing in good soil they did not require to make any reserve food! The theory was ingenious; but, unfortunately, it ignored the fact that the object of manuring, or enriching the soil, is usually to make the plant store up more reserve food.

In Ceylon this phenomenon is generally known as 'wound response,' a name which was bestowed on it by Parkin. It must, of course, have always been evident to all engaged in tapping *Hevea*, and its formal enunciation on paper made no difference to the rubber industry. In the Annual Report of the Botanic Garden, Penang, for 1897, Curtis records the result of an experiment made in June of that year, and states that 'the first day's collection yielded only half an ounce; but on renewing the cuts on seven subsequent occasions one pound of dry rubber was obtained, being an average of two ounces for each time.' In the Report for the following year he gives further details of another tapping: 'On the morning the incision was first made only a quarter of an ounce of wet rubber was obtained, but by taking a thin shaving off the lower surface of the oblique cuts on fourteen subsequent occasions, the following quantities were obtained at each operation:—$\frac{3}{4}$, $1\frac{1}{4}$, $3\frac{1}{4}$, $3\frac{1}{2}$, $3\frac{1}{4}$, 6, 9, $6\frac{1}{2}$, $8\frac{1}{2}$, 6, $6\frac{1}{2}$, 10, $8\frac{1}{2}$, 8.' In Ceylon, Willis, in 1897, found that the second tapping gave a much larger yield than the first, but subsequently the yield steadily decreased; in this instance the tappings were performed by separate incisions at intervals of one week, and the successive yields were 0·73, 1·48, 0·97, 0·80, 0·67, 0·52 ounces.

In one of Parkin's experiments eight V's were made at the base of each tree (number not stated) and a total yield of $2\frac{3}{8}$ ozs. of dry rubber was obtained; two and a half days later new V's were made a couple of inches or so above the old ones, and the total yield was $3\frac{3}{8}$ ozs. of dry rubber, an increase of about 42 per cent. In another experiment a piece of bark about an inch square was removed; two days later incisions were made near this wound, and also similar incisions, at the same level, 'away from the wound'; the average amount of latex from the former incisions was 2·05 c.c., while that from the latter was 0·90 c.c.; the effect of the previous wounding was, therefore, to more than double the yield of latex.

Parkin's chief experiment on this point was performed in 1899. Four trees were tapped by means of rows of separate V's, at intervals of three to seven days, the rows of V's in successive tappings being four to six inches apart. On the first day's tapping a row of V's was cut half way round the tree at the base on one side, and a similar row on the other side of the tree at a height of six feet. On the occasion of the second tapping similar rows were made, one above the previous basal row, and the other below the previous uppermost row. Thus the two sides of the tree were tapped at the same time, one by rows of V's from below upwards, and the other by rows of V's from above downwards. The two sets of incisions would undoubtedly affect one another, but in different degree according to the vertical distance apart.

On the first day the two rows might be regarded as independent tappings. The maximum interference would occur when the 'upward' tapping cuts were at about the same level as the 'downward'; this occurred at the seventh and eighth tappings. The following were the results :—

LATEX FROM FOUR TREES.

Each tapped by 10 V-shaped incisions at each tapping :—

When tapped.	Yield on side tapped from above downwards.	Yield on side tapped from below upwards.	Total yield per tapping.
March 25	24·5 c.c.	36·5 c.c.	61·0 c.c.
" 30	51·0 "	54·5 "	105·5 "
April 6	103·0 "	117·0 "	220·0 "
" 12	90·5 "	118·0 "	208·5 "
" 15	125·5 "	130·0 "	255·5 "
" 20	137·5 "	152·5 "	290·0 "
" 25	152·0 "	124·0 "	276·0 "
May 1	142·0 "	111·0 "	253·0 "
" 6	130·5 "	134·0 "	264·5 "
" 13	133·0 "	142·0 "	275·0 "
" 20	149·0 "	106·0 "	255·0 "
" 26	153·0 "	109·9 "	262·0 "
June 1	222·0 "	106·0 "	328·0 "
" 6	342·0 "	107·0 "	449·0 "

If the two sides are considered separately it is seen that the yield of latex is at a maximum on the first side at the seventh tapping, and on the other side at the sixth. There is a further increase on the first side at the thirteenth and fourteenth tappings, but no corresponding increase on the other side. This is due to the fact that on the first side the basal region, *i.e.*, the richest part of the stem, is tapped at the end of the experiment when a free flow has been established, and also in the wet season, while on the other side it was tapped at the beginning of the experiment and in the dry season.

The total yield shows a maximum at the sixth tapping and another at the fourteenth. It is, however, doubtful how far we are justified in drawing any conclusions from the figures of the total yield. It is evident from the table that the second maximum is entirely due to the tappings on one side only, and the reason for that increase has already been pointed out. It is usually stated that the experiment exhibits 'wound response' up to the fourteenth tapping, but it would be preferable to regard the increase up to the sixth tapping as due to 'wound response,' and the later increase to climatic effects and to tappings on a richer area. As will be perceived, the results show the combined effect of 'wound response,' tapping at different heights, and climatic variations.

In any case, if 'wound response' is a response to a stimulus caused by the act of wounding the tree, it can only influence the first few tappings. It cannot be held responsible for variations in the yield of latex or rubber when the tree is being tapped at regular intervals. Indeed, 'wound response,' if the phrase has any meaning at all, is equivalent to 'calling the rubber,' or 'accustoming the tree to milking.' It would therefore seem preferable to say that the experiment quoted above exhibits 'wound response' up to the sixth tapping.

It is most probable that Parkin was on the right track when he suggested that the state of the weather and the amount of moisture in the soil to some extent govern this phenomenon. In 1899, March and May were dry months, and April and June were wet, at Peradeniya, where the experiment was conducted. The two maxima occurred in the wet months, and it is therefore probable that their incidence was governed by the rainfall, more especially as they are maximum yields of latex, not of rubber.

Curtis demonstrated that 'wound response' was exhibited when the old cut was reopened, while Parkin show that it occurred when new cuts were made a few inches away from the old. In the language of the present day, Curtis employed the method of 'excision' now in vogue, while Parkin employed the method of 'incision.' Curtis' result shows an increase to forty times the original amount of rubber at the thirteenth tapping, but Parkin only obtained an increase to seven and a half times the original amount of latex; the increase in the yield of rubber in the latter case would be much less than that. It would appear from this that the increase obtained by excision is greater than that obtained by incision, but there are so many factors unrecorded that any such deduction would be unsound.

Parkin's experiment was begun in the dry season, and the average interval between the successive tappings was about five and a half days. Further experiments, begun in the wet season, with different intervals between the tappings, have been recorded by Bamber and Lock. Seven groups, each of ten trees, were tapped regularly, the first group daily, the second every other day, the third every third day, &c. The result of the first few tappings are given below. As it is impossible to arrange all them conveniently in one table, the results of the first four groups are given first, and then those of the last three.

	Group I. Tapped daily.		Group II. Tapped every second day.		Group III. Tapped every third day.		Group IV. Tapped every fourth day.	
	Latex.	Rubber.	Latex.	Rubber.	Latex.	Rubber.	Latex.	Rubber.
1st day	390	194	286	143	335	155	298	149
2nd day	322	159	—	—	—	—	—	—
3rd day	487	224	289	130	—	—	—	—
4th day	594	231	—	—	485	194	—	—
5th day	603	235	449	185	—	—	602	237
6th day	457	168	—	—	—	—	—	—
7th day	516	194	366	148	492	197	—	—
8th day	553	202	—	—	—	—	—	—
9th day	560	197	368	133	—	—	?	140
10th day	474	167	—	—	369	138	—	—
11th day	507	174	456	174	—	—	—	—
12th day	611	196	—	—	—	—	—	—
13th day	598	196	414	184	564	197	644	235
14th day	660	202	—	—	—	—	—	—
15th day	606	179	403	170	—	—	—	—
16th day	500	144	—	—	?	152	—	—
17th day	520	145	?	160	—	—	554	190
18th day	567	156	—	—	—	—	—	—
19th day	331	87	290	110	596	189	—	—
20th day	426	116	—	—	—	—	—	—

The above gives the results of the tappings in each of the first four groups during the first twenty days of the experiment; the yields are the weights in grams in each case. A note of interrogation indicates that the exact yield of latex is not known.

	Group V. Tapped every fifth day.		Group VI. Tapped every sixth day.		Group VII. Tapped every seventh day.	
	Latex.	Rubber.	Latex.	Rubber.	Latex.	Rubber.
1st day ...	304	117	?	135	213	114
6th day ...	336	146	—	—	—	—
7th day ...	—	—	406	175	—	—
8th day ...	—	—	—	—	412	169
11th day ...	388	146	—	—	—	—
13th day ...	—	—	538	225	—	—
15th day ...	—	—	—	—	511	188
16th day ...	?	158	—	—	—	—
19th day ...	—	—	555	220	—	—
21st day ...	?	141	—	—	—	—
22nd day ...	—	—	—	—	540	194
25th day ...	—	—	618	241	—	—
26th day ...	474	174	—	—	—	—
40th day ...	—	—	—	—	682	172
42nd day ...	516	190	625	245	—	—
47th day ...	394	155	—	—	?	141

In Groups V.-VII. there is unfortunately a period of eleven days, from the 28th to the 40th day, during which tapping was suspended. It is scarcely possible, therefore, to make any valid deductions on the subject of 'wound response' in these last three cases, as the necessary conditions were not fulfilled.

In Group I. the second tapping shows a decrease both in quantity of latex and of rubber. The maximum yields, both of latex and of rubber per tapping, are obtained at the fourth and fifth tappings, which give practically identical results. This yield of rubber is not exceeded by any other tapping of the first forty, but the yield of latex is greater at the fourteenth tapping. This second maximum, of fluid only, will be considered later. The percentage of rubber in the latex of this group falls from 49·7 at the first tapping to 25·9 at the twenty-third, after which it varies from about 25 to 35 per cent.

In Group II. the second tapping yields as much latex as the first, but the yield of rubber is less. Both the latex and the rubber per tapping are at a maximum at the third tapping. The percentage of rubber in the latex falls from 50 at the first tapping to 30·8 at the twelfth, after which it varies from about 30 to 36 per cent. The yield at the third tapping is not surpassed by any other of the first forty tappings.

In Group III. the yield of both rubber and latex at the second tapping is greater than at the first. The maximum yields are practically obtained at the second tapping, the differences between this and the third being within the limit of error. There is a subsequent large increase in the yield of latex at the fifth tapping, and a further increase in the yield of rubber per tapping (211 grams) at the thirtieth tapping (*i.e.*, 100 days from the first); but these are not due to 'wound response.' The percentage of rubber in the latex of the group falls from 46·2 at the first tapping to 31·7 at the seventh, after which it varies from about 30 to 40.

In Group IV. the maximum amount of rubber is obtained at the second tapping; the latex is greatest at the fourth tapping, but this is capable of another explanation than 'wound response.' The percentage of rubber in the latex falls from 50 at the first tapping to 34·3 at the fifth, after which it varies from 35 to 45 per cent.

In considering Groups V., VI., and VII., we are met by a difficulty which does not make itself felt in the first four groups. In these four the effect of 'wound response' is evident in the first few tappings, and in no case is it necessary to refer to more than five. The results are therefore subject only to the weather fluctuations of nine days, at a time when weather conditions are fairly constant. But if a long series of successive tappings has to be considered, then the later of these may have been performed under conditions totally different from those of the earlier; and in that case the variation in the yield may be due to environmental changes and not to any reaction to wounding. In illustration of this, the yields of Groups I. to IV., from the 12th to the 14th day, may be quoted. In each of these groups there is, with one exception, a large increase in the amounts of rubber and latex during that period, in spite of the difference in the tapping intervals. It is probable therefore that this variation is not due to 'wound response,' but to a greater rainfall about that period. (The exception is the latex yield of the seventh tapping of Group II., and it is probable that this is incorrectly stated, since the figure recorded indicates an increase of six in the percentage of rubber in the latex at a time when the percentage in every other group is decreasing.)

In Group V. the latex of the first tapping contained only 38·5 per cent of rubber, *i.e.* from 8 to 15 per cent. below the other groups. It increased to 43·4 at the second tapping, and subsequently varied from 35 to 42 per cent. The yield of rubber increased to 251 grams at the twenty-seventh tapping. On the whole, the yield per tapping of Group V. is more uniform than in the preceding groups, partly because of the longer interval and probably also because the latex was initially weaker.

In Groups VI. and VII. indications of any decided 'wound response' appear to be absent, as in both cases there is an increase in the yields until the regular tapping is interrupted. In both these groups the yields of the first forty tappings are less uniform than in Group V. In Group VII. the yield rose to 219 grams at the thirteenth tapping, and fell to 81 grams at the fortieth tapping. As the fortieth tapping occurred in April, this decrease was probably due to climatic causes.

The percentage increase or decrease in the yields of the second tapping over the first were :—

Group.	I.	I.	III.	IV.	V.	VI.	VII.
Latex ...	–16	0	+45	+102	+11	?	+93
Rubber ...	–18	–9	+25	+59	+25	+30	+48

This shows a regular increase in the amounts of rubber and latex obtained at the second tapping as the tapping interval is increased, for the first four groups; or in other words, the longer the interval between successive tappings, the greater the immediate response to wounding. But this rule does not hold good for Groups V.—VII. However, it is evident from the smaller initial yields that these three groups differ in some respect from the first four; and it will be noticed that they form a second similar series among themselves.

The following table gives the percentage by which the maximum yield of rubber per tapping during the first six tappings exceeds the yield at the first tapping, and the number of the tapping at which that maximum occurred.

Group.	I.	II.	III.	IV.	V.	VI.	VII.
Tapping	No. 5	No. 3	No. 3	No. 2	No. 6	No. 5	No. 4
Percentage increase of rubber	21	22	27	59	49	79	70

Taking the first four groups, it is seen that the response to wounding is greater the longer the interval between the tappings, and that the maximum occurs earlier as the tapping interval is lengthened. As before, the last three groups do not entirely agree with this; two of them show a greater increase than the first four groups, but the maximum yield is obtained later. But as has already been pointed out, these tappings extend over a comparatively long period, and the external conditions are therefore subject to greater variation than in the case of the first three groups.

The facts relating to 'wound response,' as far as they are known at present, may be summed up as follows: (1) Given the same interval between the tappings, the 'response' is the more immediately felt in the wet weather than in the dry, or in other words, the increase in the yield is more gradual in the dry weather. (2) At any given season the maximum yield is obtained the earlier, the longer the interval between the tappings; this of course only means earlier in the tapping series, not in actual time. (3) In any season the increase shown by the maximum yield per tapping over the initial yield is the greater the longer the interval between the tappings. Both (2) and (3) are subject to the qualification that there must be a major limit to the interval; if the interval is very long, then each tapping is practically an independent tapping, and no 'wound response' can be expected.

The percentage of rubber in the latex diminishes as tapping proceeds, but, for the first few tappings at least, this is generally compensated for by an increased flow of latex, and therefore the yield of rubber per tapping increases. In general an increased flow of latex means a diminution in the percentage of rubber. When the trees are first tapped, the percentage of rubber in the latex is about fifty; thence it falls, more or less regularly, to 25-30 per cent. in one or two day tapping, and about 30-40 per cent. when the intervals are longer. In general, the more frequent the tapping, the weaker the latex.

Parkin found that no 'wound response' was observable if only twelve hours elapsed between the tappings, but in this case the result would probably be influenced by the known difference between morning and evening tappings. He found it recognisable after an interval of a single day in some cases, but not in all. As we have already seen, there may be a decrease both in the yield of rubber and the yield of latex on the second day in daily tapping.

When a tree is tapped by means of separate incisions, the effect of wounding is observable at a distance of six inches, *i.e.*, on subsequent tapping, an incision six inches from the original wound yields more latex than one further away. It has not been determined whether 'wound response' is exhibited when tapping is changed (without any rest) from one side of the tree to the other; from theoretical considerations it would appear that any such 'response' would be small, and would be determined by the size of the tree and the style of tapping. Small trees tapped on the quarter system should not exhibit any 'wound response,' but large trees might.

It is improbable that a knowledge of the existence of 'wound response' is of any practical value, except in so far as it prevents the formation of erroneous estimates based on few tappings. The yield of rubber per annum depends on the amount obtained in steady tapping, and not on the gradual increase which takes place in the yields of the first few tappings; it cannot, therefore, be influenced by any 'response to wounding.' The idea that the interval between successive tappings is governed by the time which elapses before 'wound response' is exhibited has no valid foundation.

There is a very prevalent idea, and one which the name itself seems to imply, that 'wound response' is the result of a stimulus, or irritation, evoked by the act of wounding. According to this idea, wounding the tree sets in motion some mechanism or impulse which causes it to produce more latex. How this stimulus acts is not stated, but it is supposed to exert its influence for some considerable distance from the wound. According to one view, it causes the formation of new latex in the tubes, while, according to another, it causes the formation of new latex tubes in the fully-formed cortex between the old. The latter view may be dismissed at once. Latex tubes arise from special lines of cells, with special contents, which are laid down for that purpose by the cambium. It is impossible that 'normal cells of the cortex,' fully formed and at varying distances from the cambium, should suddenly be transformed into latex tubes. The view that new latex is formed in the tubes has something in its favour, and will be considered further below. But there is no evidence that such formation of new latex can be the direct result of wounding the tree.

Up to the present time, however, no such stimulus as has been imagined has been proved to exist, and the established facts of 'wound response' appear capable of explanation without any such supposition. There is no reason to suppose that the emptying of the latex tubes which follows wounding is other than a mechanical process, governed by the pressure in the tubes and the degree of concentration of the latex.

When the first cut is made, latex exudes because the liquid in the stem is under pressure. The pressure in the stem depends on the amount of moisture in it, and hence the flow of latex is greater in the wet season than in the dry, and greater in the morning than in the evening. Opposed to this pressure there is the force of cohesion between the latex and the walls of the tube, which, in the case of narrow tubes, offers a great resistance to the flow of liquid. The exudation of latex will continue until these two forces are in equilibrium, *i.e.*, until the pressure in the stem is unable to force the latex through the tubes. When the flow stops the latex on the cut coagulates, and the coagulation ultimately extends to the latex in the open ends of the tubes, thus blocking them with a plug of rubber. This latter process is a gradual one, and if the scrap is stripped off too soon the exudation of latex may begin again, especially in wet weather.

In the period which elapses before the second tapping, the severed latex tubes are rendered turgid again by the infiltration of water through their walls. Of course they were not emptied by the first tapping; as the latex exuded from their cut ends, more flowed in from other parts of the tree and thus kept them full, though under gradually diminishing pressure. Therefore, when water enters the tubes it merely dilutes the latex which is already there; the tubes do not become filled with water only. Consequently at the second tapping one obtains a weaker latex, in greater quantity because it is less viscid. If the interval between the tappings is short, the actual quantity of latex obtained at the second tapping may be less than on the first day, showing that the tubes have not yet absorbed as much water as they would have done if the interval had been longer. With longer intervals between the tappings, the tubes are sufficiently refilled to yield a larger quantity at the second tapping. In support of this, it may be noted that, within limits, the latex obtained at the second tapping is weaker, the longer the interval between the first and second tappings. For example, in Lock and Bamber's experiments, the following percentages of rubber were found in the latices of the first two tappings :—

Interval between tappings	1 day.	2 days.	3 days.	4 days.	5 days.	6 days.	7 days.
Percentage of rubber in latex, first tapping ...	49·7	50·0	46·2	50·0	38·5	?	53·5
Percentage, &c., second tapping	49·4	45·0	40·0	39·4	43·4	43·1	41·0

In daily tapping the percentages of rubber in the latex at the first and second tappings are practically equal ; in two-day tapping they differ by 5 per cent., in three-day tapping by 6 per cent., in four-day tapping by 10 per cent., and in seven-day tapping by 12 per cent. The initial percentage in five-day tapping is so far out of harmony with all other records that it is probably incorrect ; that for six-day tapping is unknown. It may be pointed out that these results do not support the view that the latex tubes manufacture rubber in the interval between two successive tappings, apart from what is being manufactured at the cambium.

As far as is known, the latex in an untapped tree is stagnant, *i.e.*, there is no proof of any circulation in the tubes. At the first tapping, movement will be induced only for a short distance along the tubes ; but as tapping proceeds and the latex becomes weaker, the effect will be felt further from the wound. It is probable that this cause must be added to those which account for the gradual increase in yield when a tree is first tapped.

Infiltration of water, and gravitation of the latex by purely mechanical means from other parts of the tree to the neighbourhood of the wound, seem quite sufficient to explain the phenomenon of 'wound response.' There is no reason why any 'vital' stimulus should be postulated; and anatomical evidence does not support the contention that the latex vessels in that part of the cortex which is still intact become inflated. Furthermore, all theories which require the formation of new latex tubes at the edge of the cut within one or two days are, to a botanist, quite impossible, as was pointed out by Parkin ten years ago.

If the above explanation is correct, there is nothing which warrants the use of the name 'wound response.' The phenomena observed are subject to the same rules as the irregular variations in yield which occur when the tree is being tapped regularly. The latter variations are climatic, dependent upon the amount of water present in the latex tubes, and hence upon the available supply of moisture in the soil; and the phenomena of wound response can be explained on the same supposition. It is surely unnecessary to require different reasons for the same phenomena, merely because in the one instance they occur during the course of a tapping period, and in the other at the beginning of it.

The following example affords additional evidence that the available moisture influences the percentage of rubber in the latex. One tree at Henaratgoda was said to yield much thicker latex than the others. On examination it was found that a large root on one side of the tree had been killed by *Hymenochæte;* and that the fungus had spread into the stem, killed the cortex on one side of it, and caused the decay of the greater part of the wood near the base. White ants had then eaten out the dead root and the decayed wood in the stem, leaving only a thin cylinder of sound wood up to a height of two or three feet. Evidently the consistency of the latex was due to the absence of a thickness of wood sufficient to permit of a normal supply of water.

The Manufacture of Rubber.

It is curious that, although all tapping systems assume that the tree is continually manufacturing rubber, there has been very little investigation into how and where the manufacture is carried on. Trees are tapped daily, or every other day, for a period of four years, and at the end of that time the process is to be begun again. Presumably, the yield of rubber is always remunerative; how, then, is the supply maintained? Except for the known fact that latex is formed at the cambium, the only recorded opinions are the obviously impossible statements that new latex tubes are formed in the original cortex at the edge of the tapping cut, and that ordinary cells of the cortex become latex tubes.

The only certain knowledge we have at present is that, when the cambium is manufacturing new cortex, it lays down special cells, which are destined to become latex vessels; by this method there cannot be new latex without new cortex. The contents of these cells differ from those of the surrounding cells before their walls are perforated, and they are soon converted into latex. The amount of latex produced in any given time by this method must therefore depend upon the thickness of new tissue which is added to the cortex by the cambium during that period.

During a short tapping period the layer of new tissue which is added to the inner side of the original cortex will be extremely thin, and the amount of latex in it will be very small. In a tapping period of only a month's duration it will be so small that it would make little appreciable difference to the yield, even if all of it could be extracted. Therefore, in such a tapping period it must be considered that all the rubber obtained is present in the latex tubes when the tapping begins, unless it can be shown that there is some other method by which latex is formed. In other words, if latex is manufactured only at the cambium, we can obtain no more in a month's tapping than could be obtained by stripping off the bark and extracting the rubber by a chemical process. Indeed, chemical extraction would yield considerably more because the latex tubes can never be emptied by ordinary tapping. (N.B. — These last two sentences are only by way of illustration; they are not intended to be read as an advocacy of chemical methods of extraction!)

If the tapping period extends over several years, new layers are continually being added to the inner side of the original cortex, and the latex formed in these new tissues goes to swell the general stock. How much new tissue is added per annum we do not know; no measurements or estimates have ever been made. But it must be remembered that the cortex does not increase in thickness indefinitely, and that, after it has attained its maximum thickness, layers are cut off on the exterior by the phellogen, which balance, more or less, the additions made by the cambium on the inner side. From this it might be expected that the amount of rubber per unit of area in such a piece of bark would remain, approximately, constant, except for the variations caused by tapping.

What we have to consider is, whether this formation of latex and rubber at the cambium is sufficient to account for the recorded yields. We will assume, for the sake of this argument, that latex is drawn by the tapping cut from all parts of the tree, and include the latex in the renewing bark, though it is doubtful how far the latter is drawn upon while tapping is proceeding. Practically, what we want to know is, how much rubber there is in a tree at any given time; not the exact amount, but merely a rough estimate.

Unfortunately, the only records of the percentage of rubber in *Hevea* bark relate to trees which have been tapped. Tromp de Haas records that he took samples of bark from three trees which were planted in 1883, and which, by careful tapping for five years (1903–7), had yielded 8054, 2401, and 1520 grams of rubber respectively. The thickness of the bark at a height of 1·5 metres (five feet) was 12, 11, and 9 mm., respectively. The percentages of rubber in the three barks, calculated on the dry bark, were 2·68, 2·23, and 1·62; but, calculated on the inner laticiferous 2 mm., they were 16·08, 12·26, and 7·29. The girths of the trees are not given.

Bamber and Lock record that, at Henaratgoda, 10 trees, each tapped 440 times in about 1½ years (1908–9) gave a total yield of 31,359 grams of rubber. The average circumference of the trees at a height of 3 feet was 90·5 centimetres. During this period nearly the whole of the bark on the lowest 6 ft. 6 ins. of the stem was removed. Taking 90·5 cm. as the average girth of the lowest 2 metres 6 ft. 6 ins.) and the thickness of the laticiferous bark as 2 mm., then the volume of laticiferous bark in the lowest 2 metres is 3620 cubic centimetres, and its dry weight is about 2320 grams. If this bark contains 16 per cent. of rubber (de Haas' highest figure), then the total amount of rubber in the lowest 2 metres of the stem is 370 grams. The height of these trees is about 70 feet, and they do not branch much; approximately, therefore, the area of the whole stem is about 5 times that of the lowest 2 metres, and consequently the total amount of rubber in the stem at the beginning of tapping is about 1850 grams.

As in a long series of frequent tappings the percentage of rubber in the latex is reduced from about 50 to 30, we may increase the above estimate by assuming that in the untapped bark the percentage of rubber would be 25 instead of 16. This raises the quantity of rubber to 2900 grams, not including what there is in the roots. To this must be added the amount formed at the cambium during the tapping period ; if a layer of cortex 1 mm. thick is added during that time then 50 per cent must be added to the above estimate to obtain the quantity of rubber available during that period.

The average yield per tree was 3136 grams. To meet this we have an estimated quantity of 4350 grams. But the tree is not drained dry. At the end of the tapping period its latex vessels are filled with latex containing 30 per cent. of rubber, for which we must deduct three-fifths of the original amount, viz., 1740 grams. This leaves 2610 grams, whereas the yield was 3136 grams.

The above figures are merely approximations, and in all probability they are too far removed from the truth to deserve even that title. But they serve to show some of the points to be considered in determining the problem, and in what direction our knowledge is insufficient. In particular, approximate estimates are required of the percentage of rubber in untapped bark, the percentage in the same bark after a year's steady tapping, the rate of growth of the untapped cortex, &c. In the above discussion all theories and figures have, as far as can be judged, been taken so as to make the estimate as high as possible and thus favour the idea that latex is formed only at the cambium. But in spite of that, they do not show an available amount of rubber equal to that obtained during tapping, and it would be necessary to postulate a large supply from the roots to make up the deficiency.

Moreover, the yield from these trees in 1½ years was only 3160 grams each, *i.e.*, about 7 lbs. But from trees in the same block, 12, 14½ and 15 lbs. were obtained in 12 months' tapping during 1905–6. Again, the largest Henaratgoda tree, which measures about 9 feet in circumference, is said to have yielded 90 lbs. of dry rubber in 18 months. From Culloden Estate it has been recorded that trees, probably

20–25 years old, have given 10, 18, 23 and 25 lbs. of rubber in 12 months; and on Elpitya Estate 11-year-old trees have yielded 16 lbs. of dry rubber each in the same time. From these records I think it must be concluded that output of rubber by the cambium is not sufficient to account for the whole yield, and that other parts must also be engaged in its manufacture.

It is improbable, to say the least, that new latex tubes can be formed in the cortex at a distance of one or two millimetres from the cambium. We are therefore led to inquire whether the old latex tubes cannot be refilled by the manufacture of new latex in them, as well as, or instead of, by the drainage of latex into them from other regions. There are several difficulties in the way of such a supposition. Such a method would differ altogether from the way in which the tubes are filled originally, but this objection probably does not count for much. But there are no special cells surrounding the latex tubes which could secrete latex and pass it into the tubes; a latex tube is bounded by other latex tubes, ordinary cortical cells, or the cells of a medullary ray. Moreover, although the sugars and proteids could pass through the wall of the tube, the rubber, being a colloid, could not; and therefore, whatever may happen in the case of the other constituents, the chief constituent, *i.e.*, the rubber, must be manufactured in the latex tube. If, therefore, we suppose that the old tubes are refilled by the manufacture of new rubber in them after they have partly emptied by tapping, we must imagine that some substance passes through the wall into the tube and that the rubber is subsequently formed from it.

If such a supposition is correct, it should be possible to isolate this substance from the latex of a tree which has been tapped for several months, but up to the present nothing of the kind has been recorded. There are, however, some indications that the latex does not always contain rubber. In the experiments conducted at Henaratgoda during 1905–6, when trees 15–20 years old were being tapped, it was recorded that latex which would not coagulate was obtained on 42 occasions; one tree which was being tapped from the base to a height of 30 feet yielded non-coagulable latex on 16 occasions, *i.e.*, more than 20 per cent. of the tappings, while 2 trees tapped from the base to 50 feet gave the same only on five occasions, *i.e.*, about 6 per cent. of the tappings. Again, six-year-old trees at Gangaruwa have sometimes yielded a substance which formed a translucent jelly, somewhat resembling freshly boiled starch paste, in the collecting cups. But these indications do not appear to have been followed up, though they might possibly have afforded valuable information concerning the processes which go on in the latex tubes.

It would also be expected that the refilling of the latex tubes with rubber would not be a constant, but a periodic, process, and that its periodicity would be evidenced by the yields of rubber at different seasons. If there is no periodic emptying and refilling in the untapped tree, then it must be supposed, in order to explain the theory suggested above, that whenever one chooses to tap it, whether at the age of five or fifty, the tree suddenly acquires a new faculty, viz., that of manufacturing rubber in the old latex tubes. For the published analyses of latex and the microscopic examination of the young tubes do not support the idea that in the untapped tree the latex is at first poor in caoutchouc, but becomes richer by the constant addition of caoutchouc which is manufactured in the tubes. It has, indeed, been stated that the longer the bark remains on the tree, within limits, the higher will be the percentage of caoutchouc in the latex; and that the renewed bark often acquires a thickness equal to that of the original bark long before the latex in it is sufficiently concentrated; if these statements are true, they uphold the view that the latex tubes are continually manufacturing rubber, but no figures have been adduced in support of them.

A Singapore experiment furnishes figures which purport to show that the latex from young trees (average girth 2 ft. 6 ins.) contains a lower percentage of rubber than the latex from old trees (average girth 4 ft. 2 ins.). But in the three groups of young trees quoted, the yields given are those of a second tapping period in one instance, and a third tapping period in the other two, within the year, while the old trees were tapped for the first time for three years. The former are therefore subject to the known decrease which attends prolonged tapping, while the latter are not; and for this reason the figures cannot be taken to prove anything. Moreover, the correct amounts of latex and rubber are not given, and the old and the young trees were tapped at different seasons.

In this connection, reference must be made to the '*in situ*' theory. In its original form this supposed that the ordinary cells of the cortex which was being tapped were converted into latex tubes, and thus maintained the supply of rubber, but subsequently it was modified to mean that the latex obtained in tapping any given area was derived only from the latex tubes immediately round the wound. In either form it denied the existence of any flow of latex from other parts of the tree. But in addition to the facts already recorded, the following calculation is instructive. Thirty-five lbs. of dry rubber were obtained at Henaratgoda by excising 5000 square inches of bark. Taking the thickness of laticiferous cortex as one-tenth of an inch and the specific gravity of the dry bark (with its contained rubber) as 0·64, then 11½ lbs. of bark yielded 35 lbs. of rubber in four and a half months. On the *in situ* theory the average amount of cortex which is forming latex during the tapping is 5¾ lbs. And practically the whole of this cortex is removed in the form of shavings. It is therefore obvious that the latex is not formed by the conversion of ordinary cortical cells into latex tubes; and it is scarcely possible that that weight of rubber could be formed in that period in the area operated upon. If the original cortex contained 16 per cent. of rubber, the 5000 square inches would hold less than 1 lb., and would require to be refilled about thirty-eight times in order to yield the quantity actually obtained.

The Use of Latex.

The advantages which accrue to a plant through the possession of latex are, as far as is known, so trifling that they are practically negligible. It may, in the case of *Hevea*, afford some protection from the ravages of boring beetles, because the latter are smothered in latex as soon as their tunnels penetrate the laticiferous layer; and Green has shown that insects which gnaw the surface of green shoots avoid *Hevea*, once they have found that the rubber clogs their jaws. But deer, horses, and cows will readily devour the green shoots and leaves, and slugs bite off the buds in order to drink the latex. Against fungi, latex (or rubber) affords no protection whatever. A fungus thread (*hypha*) can pass between two latex tubes without any difficulty; and although fungi in general grow best in an acid or neutral medium, the slight alkalinity of the latex is quite insufficient to prevent their growth within the latex tubes. In any case, the idea that latex has been evolved as a protection against insects, &c., belongs merely to popular natural history. It may incidentally serve such a purpose, but that is not its use to the plant, if it has any use at all.

The suggestion which has received most favour hitherto is that the latex tubes are storage places for water, which can be drawn upon in the dry season or during times of drought. But against this must be set the fact that most of our laticiferous plants, and certainly *Hevea*, do not grow in situations where they are liable to be subject to droughts; and laticiferous plants which do inhabit dry regions possess other adaptations which prevent too rapid desiccation. Moreover, in *Hevea*, the response to changes in external conditions is too rapid, *i.e.*, the latex tubes lose their water too readily, to admit of the acceptance of this view. For the latex is more concentrated in the evening than in the morning of a fine day.

Of more importance is the question whether latex is a waste product or a reserve food. Opinions on this matter appear to be subject to cyclic changes; a few years ago the waste-product theory held the field, but the reserve-food theory is now coming into favour. As no experiments have been made on *Hevea brasiliensis*, only the facts which have been proved for latices in general can be outlined here. It has frequently been observed that there is no reason to suppose that all latices have the same function, an aphorism which is apt to obscure the fact that we do not know the function of any one of them. But if a definite function can be proved for any one kind of latex, or any constituent of it, a clue might be furnished which could be turned to advantage in the investigation of other latices. Hence it is not a waste of time to record what has already been proved, even if it does not refer specially to *Hevea*.

Early investigations on latex dealt chiefly with its composition. In different plants it may contain fats, sugars, resins, caoutchouc, proteids, or starch. In *Hevea brasiliensis* it contains forty to fifty per cent. of caoutchouc, about three per cent. of proteids, and traces of sugar. Treub, working with a plant whose latex contained starch, claimed that the starch disappeared from the latex tubes if the plant was kept in darkness; but Schimper, who repeated the experiment, stated that this did not occur, and the latter statement was confirmed by Groom.

Scott considered that as the young plant (*Hevea*), immediately on germination, possesses a well-developed laticiferous system with cross walls already perforated, it was impossible to doubt that this tissue played an important part in conveying the food substances which had been stored in the seed to the developing parts of the plant; he considered that this view was supported by the fact that in the earliest stages the wood, which usually conveys food up to the stem, is very poorly developed, whilst the laticiferous tissue is well advanced. At this stage, *i.e.*, when the stem is about an inch in length, all parts of it are filled with starch (*i.e.*, with food), except the latex tubes; there is no starch in the latter.

Schullerus, working with *Euphorbia lathyris*, claimed that the latex became more watery as the plant grew older. He noted that the laticiferous system was prominent immediately after germination, and suggested that the latex might be of use as a food during the early stages of the plant. This abundance of latex in the germinating seedling, however, fits in equally well with the 'waste-product' theory, as at that time chemical changes in the plant are especially active.

During the current century the subject has been investigated by Molisch, Kniep, and Mdlle. D. Bruschi. Molisch (1901) favoured the idea that latex is a circulating cell sap which contains a certain amount of available reserve food. Kniep (1905) came to the conclusion that latex was not a reserve food, and that there was no circulation. In one of Kniep's experiments a branch of *Ficus elastica* was girdled down to the wood; and it was found that manufactured food was not transferred from the upper part of the branch to the part below the girdle. Now, *Ficus elastica* has latex vessels in the pith, and these are not affected by girdling; the experiment, therefore, would seem to prove that the latex vessels cannot transfer food from one part of the stem to another. Fitting (1909) considers that Kniep's conclusions are correct.

Mdlle. D. Bruschi (1909) examined various species of *Euphorbia* as well as *Ficus carica*, *Ficus pseudocarica*, and *Ficus elastica*. On the whole she agrees with Molisch, that latex contains reserve food materials. She found that the quantity of latex, its pressure, and its aspect, varied with the season in the figs (*Ficus carica, Ficus pseudocarica*), but not in the Euphorbias (*Hevea* belongs to the Euphorbias). In the two figs named the proteins are abundant, but they vary in quantity with the season; in *Ficus elastica*, and the Euphorbias, they are scanty. The most important food components in latex are the fats—in all probability they are the only reserve food—and the amount present varies with the periodic changes in the manufacture of food by the plant. When the plant is subjected to extreme hunger—either by keeping it in darkness, or in air free from carbon dioxide—for a long time, the substances in the latex are partly absorbed; the fat disappears first, then the proteid. But starch, in those latices which contain it, is not altered; neither is the rubber in *Ficus elastica,* nor the resin in *Euphorbia*.

It would appear from this that although other substances in the latex may be reserve food, the rubber is not. Under these circumstances, since the quantities of other substances in *Hevea* latex are small, its food value is practically negligible.

It has been argued that the presence of enzymes in *Hevea* latex shows that it is a reserve food. Mdlle. Bruschi finds that in the fresh latex of plants other than *Hevea* there is generally a weak peroxydase, but no oxydase; but when the plants have been starved a strong oxydase is present. A pepsin (*i.e.*, an enzyme which attacks proteids) was found in the latex of *Ficus carica* and *Ficus pseudocarica*, and a trypsin in all the latices examined; but in spite of the fact that the fat is consumed, a lipase (*i.e.*, an enzyme which attacks fat) was not obtained. Amylase (*i.e.*, a starch-attacking enzyme) is, when present, too weak to be of use. The presence of enzymes is, therefore, general, and not peculiar to *Hevea,* and cannot be taken to prove anything.

According to recent reports, a newly-discovered laticiferous plant from Madagascar contains rubber only at certain seasons. If this is correct, it might be a case in which the rubber was a reserve food, stored up during one season and consumed during another. On the other hand, this conclusion does not necessarily follow, since the difference might depend on structural changes in the plant.

The question whether the rubber in *Hevea* latex is a reserve food or not is a most important one for the planter.

The probability at present is that it is not, since there is no known ferment capable of transforming the rubber into a soluble product, and as long as it is insoluble it cannot be transferred from the latex tubes to other parts of the plant. But if it were a reserve food it would be expected to share the variations which other reserves undergo, being stored at certain seasons and consumed at others; and, in that case, it would be wrong to tap all through the year. Ridley states that the yield of rubber is poorer when the trees bear a heavy crop of seed—a fact which would tend to support the reserve food theory, if correct. On the other hand, results at Henaratgoda cannot be said to confirm that view, and it has been alleged that on one estate in Ceylon the yield is always heaviest in June or July, *i.e.*, when the seed is being matured, quite independently of the rainfall. Moreover, Witt, at the Rubber Exhibition of 1908, stated that in Brazil the trees blossomed in the middle of the tapping—*i.e.*, the trees were tapped when the reserve food was nearly at a minimum.

It is improbable that this problem will be solved by the results obtained in ordinary tapping; and all such which have been quoted in support of one view can be equally well interpreted in favour of the other. The quantities of rubber obtained by tapping the same tree at different seasons do not necessarily indicate the relative amounts of rubber in the bark at those times, since the flow of latex depends on the weather conditions, and, in ordinary estate routine, is influenced by the previous tappings. What is required is an exact determination of the amount of rubber per unit area of bark in the same untapped tree at different seasons.

The following point, which tells against the reserve food theory, has, as far as I am aware, not been previously noted. It is generally believed that young trees which have been tapped increase in girth, over the tapped area, more rapidly than untapped trees of the same age. But if the latex contains reserve food, the former have less food at their disposal than the latter. Therefore the trees with the least reserve food make the greatest growth!

When *Hevea* leaves are shed at the beginning of the wintering period they contain a certain amount of rubber. This may be demonstrated by carefully peeling off a thin strip of tissue from the midrib on the back of the leaf, when strands of rubber will be seen stretching across the angle between the strip and the remainder of the midrib. Now, according to previous investigations (on leaves other than *Hevea*), before a tree sheds its leaves all the potash, phosphoric acid, starch, &c.—in fact, everything which can be of further use to the plant—is transferred from the leaves to the stem; the dead leaves retain only waste products. Therefore, the fact that the rubber is cast off in the dead leaf tends to support the view that it, too, is a waste product. It is, however, only fair to add that more recent investigations have thrown some doubt upon the belief that a dead leaf contains waste products only.

CHAPTER III.

The Strength of Plantation Rubber.

No question is of greater importance to the rubber planter than that of the strength of his product; and there is none on which greater differences of opinion exist. It is certain that the average plantation product is not equal to Hard Para, although it is supposed to be obtained from the same tree; and, this being the case, it behoves all planters and agricultural departments interested in the rubber industry to make every possible effort to find out where and how the difference arises. Up to the present very little attempt has been made to solve this problem by scientific methods. There is scarcely anything to be gained by a comparison of ordinary Hard Para with ordinary Plantation *Hevea*, except another instance of the superiority of the former. Such knowledge leaves us as we were. It is merely an empirical result, not the result of a controlled experiment; and no one is any the wiser as to the cause of the difference. If any decided advance is to be made, empiricism must be avoided. If experiments are confined merely to trying different methods of manufacture at random, that is pure empiricism. One of these methods may be successful in removing the difference. If so, well and good; even though no one knows why it is successful. But if a method is unsuccessful, no one knows where and why it failed; and the failure therefore teaches nothing. Empiricism is justified by success; but in case of failure, its failure is most absolute: for nothing can be learnt from the failures of haphazard empiricism, and the time spent has been completely wasted. A lucky guess may solve the problem, but in the majority of cases, a properly conducted investigation, in which the various factors are distinguished and their separate effects determined, will yield results in a shorter time.

Among the factors which may influence the quality of plantation rubber may be mentioned the age of the tree, the season and the method of tapping, and the method of preparation. The ultimate test of quality must of course be made by the manufacturer; but in the tests which he has made hitherto these factors have been as a rule unknown. His samples may have been obtained from trees of different ages, at different seasons of the year, by different tapping systems, and prepared by different methods. Moreover, they may have been stored for different periods and under varying conditions. Therefore the results of his test do not teach us anything; and, until quite recently, no attempt was made to supply him with samples which would be subject to the variation of only one factor. Up to the present, therefore, there is no real knowledge of how the strength of plantation rubber is affected by any single factor.

The following conclusions were published in the *Ceylon Observer* of June 18th, 1909.

(1) The quality of the rubber is not affected by the age of the tree which yields the latex.

(2) The quality is the same from natural or renewed bark.

(3) The use of formalin in the latex does not affect the quality of the rubber.

(4) The quality of rubber obtained when acetic acid is used appears to be at least equal to that obtained by natural or spontaneous coagulation.

(5) Vacuum drying, as at present carried out on the estates, lowers the quality of the rubber, but this may be modified by curtailing or lengthening the time it is in the vacuum press.

To these conclusions was added that no decision had yet been arrived at as to the keeping qualities of the rubber after vulcanisation.

The above results were apparently obtained from an examination of specimens from Lanadron Estate. The age of the trees and other particulars were not stated; and it is therefore doubtful how far they are generally applicable.

THE AGE OF THE TREE.

Parkin stated that 'the rubber collected from green stems and leaves has not the same quality as that procured from the trunk. It is somewhat adhesive, with less elasticity and strength. In fact, the essential properties of caoutchouc are not well exhibited by this rubber obtained from the younger parts of the plant. The same is true for the rubber obtained from the unripe capsules, the outer part of these being especially rich in latex. Whether the defect is in the caoutchouc itself, or due to some other matter present, has not been definitely ascertained. Most likely it is due to a difference in the chemical nature of the globules of latex themselves. When some of this latex is boiled well with alcohol and then filtered, and the clear filtrate mixed with water, only a slight milkiness is observed, pointing to a trace of resin ; the latex of the trunk behaves similarly, just containing a trace of resin. Hence the poor quality of this rubber from the young parts does not seem to be brought about by the presence of a larger amount of resin or other matter soluble in alcohol.'

Seedlings a few days old, before their leaves were formed, were cut up in a dilute solution of ammonia which was afterwards neutralised by the addition of acetic acid ; several hundred seedlings were treated, but only a thin film of rubber was obtained. This rubber was slightly adhesive, and, after being kneaded into a small cylinder and dried, showed very little elasticity.

The substance in the latex tubes of green shoots has been erroneously termed viscin. Spence, in pointing out this error, states that what actually occurs in the young latex tubes are globules of gummy pectin-like bodies which yield sugar on hydrolysis by acids. If this is true of the young latex tubes of the stem of *Hevea*, it has an important bearing on some methods of tapping, *e.g.*, on the use of the pricker, since the latter extracts the youngest latex. It may be noted that the rubber which coagulates naturally on green shoots appears to be of fair quality.

Rubber from two-year-old trees is slightly adhesive, soft, and has scarcely any elasticity, and that from four-year-old trees is generally weak. What is usually regarded as marketable rubber is obtained in the fifth year, and except for a comparatively small quantity, plantation rubber is now being obtained from trees five to fifteen years old. The oldest trees in Ceylon are now thirty-five years old. At the Rubber Exhibition held at Peradeniya in 1906, biscuits from these old trees, which had not been entered for competition, were examined by the judges, and it was stated that they were considerably stronger than any of those exhibited.

Ridley has obtained reports on two specimens of *Hevea* rubber, one from trees twenty-five years old (A), and the other from trees eight years old (B). Both had been prepared in the same way, by coagulation by exposure to smoke. Analyses made at the Imperial Institute give :—

	A.		B.
Moisture	9·0	...	8·8
Caoutchouc	80·2	...	82·0
Resin	4·7	...	4·7
Proteids	5·3	...	3·4
Ash	0·8	...	1·1

The Indiarubber, Gutta-percha, and Telegraph Works Co., Ltd., reported that sample A lost 23·6 per cent. on washing, and sample B 16·1 per cent.; and that *though sample A had been overheated in the process of smoking, it was superior to B.* The samples were not considered equal to Fine Para by 8 to 15 per cent.

The Harburg-Vienna Indiarubber Works found a loss of 21·8 per cent. on washing in sample A, and 13 per cent. in sample B.

Physical tests gave :—

	Viscosity.	Breaking Strain.	Stretch.	Adhesiveness.
Singapore A.	21·4	32·5 ks.	350 %	9·0 ks.
Singapore B.	21·9	20·0 „	328·75 %	9·5 „
Hard Para	59·8	21·0 „	362·5 %	10·0 „

They reported that *sample A gave a better result than B*, and that the samples proved that the material of the Botanical Garden was quite equal to fine hard-cured Para.

Having regard to the facts detailed above, it is scarcely possible to accept the statement that the quality of the rubber is not affected by the age of the tree, without further details of the evidence on which that opinion was based. It is of course evident that there must be a minor limit to the age of the tree, and it can only be supposed that the samples on which that statement was based were taken from trees which did not differ greatly in age. An examination of samples in Ceylon, all coagulated by acetic acid, certainly leads to the conclusion that rubber from old trees, twenty to thirty years old, is stronger than that from trees six to eight years old.

Bamber has analysed the rubber from trees of different ages, with the following result:—

	2 yrs. old.	4 yrs. old.	6 yrs. old.	8 yrs. old.	10–12 yrs. old.	30 years old.
Moisture	0·70	0·65	0·55	0·85	0·20	0·50
Ash	0·50	0·30	0·40	0·14	0·22	0·25
Resin by acetone extraction	3·60	2·72	2·75	2·66	2·26	2·32
Proteids	4·00	1·75	1·51	1·75	2·97	3·69
Rubber	91·20	94·58	94·79	94·60	94·53	93·24

It is evident that there is very little difference in the chemical composition of these rubbers as determined by the methods of analysis now in vogue. The percentage of resin is practically the same from four to thirty years; and though the proteid in the rubber from two-year-old trees is more than double that in the rubber from four-year-old trees, it is only slightly greater than that in the rubber from thirty-year-old trees. There is 'nothing that would definitely indicate to what strength and resilience are really due.'

Parkin has recently suggested that the reason why rubber from young trees is weaker than rubber from old trees may be due to the fact that the former may contain a large proportion of rubber formed in 'primary growth.' 'In such young trees the primary laticiferous tubes will still be yielding some latex, which will mingle with that from the secondary tubes, giving an intermediate product.'

It is easy for the planter to satify himself that secondary growth of the stem sets in when the *Hevea* plants are only a few weeks old. When they are taken out of the nursery and stumped, all that is living when planted out is 'secondary growth'; the primary wood is buried in the middle of the stem, and the primary cortex (most, if not all) forms the brown layer on the exterior. The new shoots which spring from the stump pass through a stage of primary growth, but practically only in so far as the cortex is concerned. For all practical purposes (since he deals chiefly with the cortex) the planter may reckon that green shoots are in the primary stage of growth, but everything else is in the secondary stage.

Now, it is quite evident that there is no living vestige of the primary cortex on the lower six feet of a four-year-old stem. Therefore the planter cannot obtain latex formed in primary growth, unless in tapping he draws upon the whole tree, up to and including the green shoots. Such an idea is quite contrary to the theories of our rubber experts (the length of a cigarette was one estimate of the distance from which latex is drawn); and even those who maintain that latex is drawn from a considerable distance are not prepared to extend that distance to the top of the tree. That, however, is the only way in which rubber formed in primary growth can be obtained by the usual tapping methods. It has been shown that the rubber formed is not all stored in the tree, but that some is cast off with the dead bark; that is the fate of the rubber produced in the primary growth of the cortex of the lower six feet of the stem.

From a botanical standpoint, Parkin's theory is improbable. If it were correct there should be a progressive decrease in the strength of the rubber as the tree is tapped, for it can hardly be supposed that the primary rubber is drawn upon during the first tappings. This, however, would not provide a decisive test of the theory.

The Age of the Rubber.

The outer brown bark, the true bark, is formed from the laticiferous cortex by the activity of the phellogen. If this brown bark is powdered up in a mortar and extracted with carbon bisulphide an appreciable quantity of rubber is obtained. When the laticiferous cortex is converted into true bark, the latex in it dries up and leaves the rubber in the outer dead layer. The experiment quoted has only been performed qualitatively, *i.e.*, it only shows that some rubber is discarded with the dry brown bark. Obviously it is necessary to compare the actual weights of rubber in equivalent volumes of cortex and bark before it can be affirmed that *all* the rubber in the cortex is rendered unavailable when that cortex is transformed into the corky brown bark.

The preceding facts contradict the widely prevalent theory that all the rubber which is formed in the stem of a tree accumulates in the laticiferous tissue until the planter chooses to tap it; that if he does not tap until the tree is eight years old he will obtain all the rubber which was in the tree when it was, say, six years old, plus the amount which has been formed during the two succeeding years. Such an idea assumes that the rubber in the outer layers of the cortex is transferred inwards to the inner cortex whenever the outer layers are converted into dead dry bark, whereas, as we have already seen, it remains, in part at least, in the dry bark, and is put out of the reach of any tapping system. Some of the rubber which was in the tree at the age of six is undoubtedly rendered unavailable before the tree is eight years old.

It follows from this that the rubber in a six-year-old tree is not six years old, and that the rubber from an eight-year-old tree is not necessarily older than that extracted from a six-year-old tree. The laticiferous cortex is the balance between the amount of tissue constructed externally by the cambium, and the amount cut off by the phellogen; and, as far as is known, the rubber in it is the same percentage of the total rubber formed in the stem during the life of the tree as the cortex is of the total external tissues. When the trees are old there can be little difference in the real age of the rubber in their cortices: there is no reason to suppose that any of the rubber in the cortex of a thirty-year-old tree is older than that in the cortex of a twenty-year-old tree. Obviously the idea that the rubber from a thirty-year-old tree is stronger because it has been stored in the tree for a longer time is incorrect.

But other considerations make it improbable that the rubber from the older trees owes its superiority to a prolonged storage in the cortex. The latex is most abundant in that part of the cortex nearest to the cambium, and in every method of tapping the bulk of the yield flows from that zone. But this is the most recently formed part of the cortex, and the rubber in it is the youngest rubber. Therefore, whatever the system of tapping, the rubber obtained is the youngest rubber in the stem, and is practically the same age, whatever the age of the tree, at least after the first five years.

If we accept the view that the rubber from older trees is stronger than rubber from younger trees, we must conclude that the rubber manufactured by a tree at the age of, say, twenty, is stronger than the rubber manufactured by the same tree at the age of ten, though it may be admitted that from a botanical standpoint there does not appear to be any reason why this should be so.

The System of Tapping.

It has been asserted that the quality of the rubber from any tree is influenced by the system of tapping. Rubber obtained by tapping every three days was said to be of better quality than that obtained by tapping every day, or every other day. These statements have not been supported by any experimental evidence, and it would seem improbable that they are correct.

No tests have yet been made of rubber from the same trees at the beginning and end of a tapping period, *i.e.*, of the rubber obtained from the earliest and latest tappings on the same side of the tree. As is well known, the percentage of rubber in the latex is considerably reduced as tapping proceeds, and it is conceivable that there might be some deterioration in the quality of the rubber also. In that case the view that three-day tapping gives a rubber of better quality than two-day tapping might be simply a misinterpretation of correct observations, for the stage at which deterioration began would be reached sooner by the latter system than by the former, and the difference might be due to the greater number of tappings and not to the interval between the tappings.

Ridley claims that a change in the quality of the rubber occurs when the yield of latex at the beginning of tapping rises to a maximum, or during seasons of heavy rainfall. After stating that the colour of the coagulated rubber changes from yellow to white at about the sixth tapping, and that the coagulated rubber is white at seasons of heavy rainfall as a consequence of excessive moisture, he adds that in his opinion the coloured rubber is the strongest. According to this view, a tree may yield strong rubber one day and weaker rubber the next.

The Method of Preparation.

Spence has proved that the quality of the rubber may be influenced by the method of preparation. Two samples of rubber were prepared from the same sample of *Funtumia* latex, the one by adding sulphuric acid and the other by adding absolute alcohol and heating. The latter yielded a sample of rubber of good quality, whereas the former melted into a soft, resin-like paste.

The method of preparation now in vogue in the East, viz., by the addition of acetic acid, was based on Biffen's announcement that the smoke of burning palm-nuts used to coagulate Para rubber contained acetic acid. That the burning nuts should give off acetic acid was only to be expected, since the destructive distillation of wood is one method of manufacturing it. If there is no other active substance in the smoke except acetic acid and creosote, then it would be a waste of time and money to establish plantations of those palms, as is often suggested, in order to use the nuts for smoking rubber, for any other wood smoke would serve the purpose equally as well. All that is required is that the burning material should give off a dense smoke.

In addition to acetic acid, Parkin experimented with sulphuric, hydrochloric, nitric, oxalic, tartaric, and citric acids, as well as with sodium chloride, alum, ammonium sulphate, magnesium sulphate, and mercuric chloride. Acetic acid was chosen because it permitted wide variation in the quantity which would effect complete coagulation, not because it produced a stronger rubber. Unfortunately, no tests of the quality of the rubber produced by these different coagulants have ever been made. It may be deduced from Spence's experiment that sulphuric acid yields a weak rubber; though, on the other hand, Morisse, after several experiments, finally adopted a mixture of sulphuric and carbolic acids.

Ridley asserts that 'the difference in the physical tests and appearances of rubber from young or old trees coagulated with such reagents as acetic acid is not, or scarcely, perceptible.' In other words, the use of acetic acid reduces all *Hevea* rubber to one common level. This is not borne out by Ceylon experience.

Though Parkin experimented with hydrochloric acid, he did not try the other acids of the same series, viz., hydrobromic, hydriodic, and hydrofluoric. These last three are of little commercial importance, and, consequently, are not generally stocked. Moreover, hydrofluoric acid is dangerous to handle, as it quickly cauterises the skin, producing a painful, slow-healing sore; and it must be kept in leaden or gutta-percha bottles, because it rapidly attacks any substance of which bottles and basins are usually made. The range for complete coagulation by hydrochloric acid is small; that for hydrofluoric acid is not known, but from chemical considerations it should be even smaller. It is claimed that the use of hydrofluoric acid allows the rubber to retain from nine to ten per cent. of water, and that the rubber has, consequently, a great degree of elasticity; also that it gives a purer rubber, which can be packed as prepared without special drying. Experiments with this coagulant were conducted in Ceylon in September, 1909, but the results have not yet been published.

The defect in plantation rubber prepared by the use of acids has led to experiments in imitation of the Brazilian method, viz., by smoking. Ridley has demonstrated that rubber can be prepared in this way, from trees in the East, almost equal to Hard Fine Para; the manufacturer's report on one sample states that the material is quite equal to fine hard-cured Para, while of another it was stated that, 'in quality and general behaviour, this rubber is extrémely like Hard Fine Para in tensile strength and in power of recovery, but is slightly softer, and requires a different vulcanising heat.' As demonstrations of the advantage of the smoking process, however, these Singapore experiments miss the chief point, for no sample of unsmoked rubber from the same trees was submitted for comparison. If the rubber of every other tapping had been smoked, while that of the intervening tappings had been coagulated with acetic acid, the advantage, or otherwise, of the smoking process would have been proved beyond question. As it is, the fact that this rubber came from trees twenty-five years old must also be considered. It is improbable that the Brazilian method of smoking rubber will ever be adopted on Eastern plantations, though some modification of it might be. Up to the present, however, the smoking machines which have been put on the market are of too limited a capacity, and the number required to treat the latex of an estate in bearing is so great that the cost is prohibitive.

It was pointed out at the Rubber Exhibition of 1906 that Para rubber is built up in layers, *i.e.*, that the latex is poured over the ball of rubber and coagulated on the exterior. Each layer is shrunk on to the original lump, and the inner layers are therefore compressed by the outer. It has been suggested that this constant pressure has something to do with the production of a stronger rubber, and the success of the various 'block' forms of rubber would appear to support the suggestion.

Naturally-coagulated rubber appears to be stronger than that coagulated with acetic acid. This is especially noticeable in the case of the pads of rubber which are sometimes formed between the cortex and the wood of the stem. But no tests of such rubber are on record, and it is therefore impossible to say whether it is in reality stronger.

Many other suggestions have been put forward to account for the difference between plantation *Hevea* and Fine Hard Para. It is said that plantation rubber is dried too much, and that it would be more elastic if it were allowed to retain about ten per cent. of moisture. Experiments with 'wet block' rubber were begun in 1906, but they were not comparative, and, apparently, they were not carried up to manufacturing tests. Again, it is argued that the acetic acid process removes too much proteid—according to Bamber's analyses only fifty per cent. of the proteid in the latex is present in the coagulated rubber—and that Hard Para is superior because it contains all the proteid. On the other hand, it is suggested that the proteid lowers the quality of the rubber, and therefore it ought to be removed. Consequently we find that inventors of patent coagulants are at a loss to know what special advantages to claim for their inventions. One claims that his special nostrum removes all proteids, and therefore the resulting rubber is superior; while another asserts that his includes all the proteid—with the same result. All of which conflicting claims serve to illustrate how little is really known about the subject. We need comparisons—manufacturing tests—of 'wet rubber' and dry rubber collected from the same trees during the same period; of rubber plus proteids, and rubber minus proteids prepared from the same latex; of smoked rubber, and acid-cured rubber from the same latex; of block rubber, and biscuit rubber from the same trees, &c., before we can assign any value to these differences. Added to these must be tests of rubber from trees of different ages, collected and coagulated in the same way and stored for the same time; of rubber from the same trees coagulated by different reagents; of rubber from the same trees at various stages of tapping, &c. Only by determining the effect of each separate factor on the plantation *Hevea* will it be possible to discover why it is inferior to Hard Para. And, until the cause of this inferiority is known, attempts to remedy it are pure empiricism.

One theory, for which we have waited until 1910, removes all necessity for further research, if accepted. It was first enunciated in private by an American rubber buyer, and subsequently found its way into the Ceylon Press. According to this theory, Hard Fine Para is the product of trees which grow on the higher regions inland; the rubber obtained on the lower rivers is not Hard Para, and is the product of a different variety of *Hevea brasiliensis;* therefore Eastern Plantation Rubber is not equal to Hard Para, because it is not the product of the same tree. But, as in the case of the various theories already referred to, there is no reason why this one should be accepted without further evidence. It has since been contradicted by Wickham, who brought the seed from Brazil.

The keeping Qualities of Plantation Rubber.

Almost of equal importance to the question of the strength of plantation rubber is that of its keeping qualities. It is generally stated that plantation rubber as ordinarily prepared will not keep, but deteriorates rapidly on storage. These two questions are often confused, or rather, they are generally regarded as interdependent, though in all probability they are quite distinct. Instances are not wanting in which plantation rubber has remained good after several years' storage, but as a rule the several factors which may have produced this result have not been recorded, and therefore their evidence is inconclusive. Ridley has furnished several examples, of which most of the data is available. A small block of rubber, allowed to coagulate naturally in a square tin, in (or about) 1890, was firm, clean, sound and good, though quite black, twenty years later; this rubber was obtained from the old trees at Singapore, which would then be about thirteen years old. Again, a piece of rubber made in a saucer in 1893—one of the first biscuits—was also sound and good in 1910. On the other hand, a biscuit made at Singapore in 1890, without acid, was quite hard and stiff, though still pale, yellowish white, in 1910; and black specimens made in 1893 and 1894 were then fairly sound and elastic, though showing signs of deterioration.

Parkin's biscuits, made in Ceylon in 1898-99 by the hot acid treatment with the addition of creosote, were said to be sound in 1906; the trees from which these were obtained may have been twenty-two years old, but this cannot be stated with certainty.

Of the specimens exhibited at the Ceylon Exhibition of 1906, those which were available in 1910 proved that only the 'block' forms had survived the four years' storage without considerable deterioration. Generally speaking, the remaining samples of biscuits represented a low grade rubber, while others kept for exhibition purposes had already been discarded as worthless. On the other hand, Lanadron block, made in 1906, was in good condition, Gikiyanakande worm block of the same date was valued at Rs. 4.30 per lb. in Colombo (Hard Para being six shillings and threepence, London), while Ingoya pressed biscuits of the same date were described as very good in every way, and evoked the query, 'Why don't estates generally make it like this nowadays.' It may be taken for granted that the exhibits were the produce of the oldest trees available in every case, and therefore it would appear probable that the biscuit form offers greater facilities for deterioration than the block, no matter how the block is made. This evidence tends to support the suggestion that the rapid deterioration of biscuits is due to the greater surface, relatively to their bulk, which is exposed to atmospheric influences. On the other hand, it was stated by Colombo brokers that the resiliency and strength of Syston rubber biscuits, also made in 1906, had been in no way impaired up to December, 1910.

Ridley lays down the general rule that unsmoked rubber of any grade perishes much earlier than smoked samples, but no evidence is offered in support of that. Derry made smoked rubber in Perak in 1897, but there does not appear to be any record of the keeping qualities of his samples, as compared with unsmoked rubber from the same trees.

The fact that naturally coagulated rubber has kept sound and good for nearly twenty years would appear to negative all those theories which would attribute the deterioration of rubber to the decomposition of the proteid in it. For naturally coagulated rubber originally contains all the proteid, and the latter undergoes extensive decomposition. No doubt much of it decomposes during coagulation, but, granting that, it is clear that the products of decomposition cannot have much effect on the rubber, for they remain to a great extent enclosed in it without injuring it. Probably an investigation into the processes and products of decomposition which occur when rubber is allowed to coagulate naturally would afford valuable information. At present very little is known about the putrefactive changes in *Hevea* latex, and the explanations given as to the growth of bacteria and moulds and the subsequent action of these on the rubber are obviously incorrect.

The keeping properties of Hard Para are usually attributed to the process of manufacture, it being supposed that the smoke acts as an antiseptic and prevents decomposition. Hence it is recommended that antiseptics, *e.g.*, creosote, mercuric chloride, &c., should be added to plantation rubber for the same reason. But it must be pointed out that the existence of putrefactive changes in plantation rubber which influence the caoutchouc in any way is a pure assumption, and that where the greatest putrefaction of the proteid has taken place, *i.e.*, in naturally coagulated rubber, the keeping qualities are unimpaired. A reduction of strength or durability of the rubber because of the action of bacteria on the proteids could only be explained on the theory that the strength of the rubber depends upon a network of proteid in it, not on the actual caoutchouc. When bacteria and moulds have been found on *Hevea* biscuits, &c., the rubber has not necessarily shown signs of deterioration ; and in most cases of softening or tackiness, bacteria and moulds are not present. It is quite possible that creosote may prevent deterioration of rubber, but that it does so by virtue of its antiseptic properties certainly does not appear probable.

Resins in Rubber.

Analyses have already been quoted which show that the difference in quality between samples of rubber from old and young *Hevea* cannot be attributed to the percentage of resin which they contain. The following examples serve to demonstrate that, with other kinds of rubber also, the value is not necessarily influenced by the percentage of resin. The samples were selected and valued by a London broker for exhibition at the Ceylon Rubber Exhibition of 1906, and the analyses were performed by Mr. M. K. Bamber at the close of the Exhibition:—

	Value.		Percentage of Resin.
	s.	d.	
Assam *Ficus*	4	3	12·80
Darjeeling *Ficus*	4	3	8·12
Assam *Ficus*	3	9½	5·70
Madagascar	3	8	10·54
Mangabeira	3	1	6·04
West Indian Plantation (*Castilloa*)	4	6	11·54
Mexican Plantation (*Castilloa*)	4	6	18·54
Ecuador Roll (*Castilloa*)	3	8	6·04
Ecuador Scrap (*Castilloa*)	3	8	5·34
Colombia smoked sheet (*Castilloa*)	3	0	7·4
Peruvian Tails	3	4	1·84
Java Plantation (*Castilloa*)	4	2	18·86
Mozambique Stickless	4	5	7·58
New Guinea...	3	6	4·54

CHAPTER IV.

Physiological Considerations.

The functions of the various parts of the tree have already been briefly referred to, and we may now endeavour to obtain a connected picture of all the processes which are carried on in it.

These processes have been investigated for many years in plants of various kinds, and from the results of such investigations the fundamental facts of plant physiology have been established. There is nothing in *Hevea* which would make it an exception to the general rule—its laticiferous system is an addition to a normal plant structure, not a substitute for some other tissue—and, consequently, the laws of plant physiology may be applied to it without fear of error.

The Manufacture of Food.

The earliest experimenters in this subject grew plants in pots which contained a weighed quantity of soil, and supplied them with water only. After several months, or years, the plants and the soil were weighed separately, and it was found that, though the plants might have gained considerably in weight, the amount lost by the soil was practically negligible. They therefore concluded that the whole of the substance of the plant had been obtained, not from the soil, but from the water. That conclusion was incorrect, because the chemistry of their day was not sufficiently advanced to permit correct deductions to be made. The chief constituent element of the dried plant is carbon, and the whole of this carbon is obtained from the atmosphere. The roots supply water, nitrogen (in the form of nitrates or ammonium salts), and minute quantities of mineral salts; but the carbon is obtained from the carbon dioxide of the air, which is taken in by the leaves. It is important that this last fact should be clearly understood.

Each leaf is a factory, in which the raw materials absorbed by the plant are converted into food; for all practical purposes we may assume that food is manufactured only in the leaves. Hence, if a tree is to develop normally and vigorously, it is of the utmost importance that it should be furnished with a well-developed crown of leaves, in which a large supply of food may be elaborated. In *Hevea* this is all the more important, because tapping operations, whether by incision or excision, make excessive and abnormal demands on the food supply for the construction of the renewing bark, and probably for the renewal of latex also. When the foliage of a *Hevea* is diminished, either by lopping its branches or by the shade and interference of the branches of adjoining trees, its capacity for manufacturing food is diminished in the same ratio, and its growth and the rate of the renewal of the bark must be slower. It is curious that lopping should have been recommended as a remedy for branch interference, seeing that the physiological effect of the two is the same; and it is also curious that discussions on the mutual interference of trees should usually be confined to interference of their roots, which is of small account in comparison with the effect produced by the interference of their branches.

Root Activity. The root system absorbs, from the soil, water which contains inorganic salts, *e.g.*, nitrates, sulphates, phosphates, &c. This absorption takes place only through the root hairs near the tips of the white growing rootlets, and it ceases when the roots cease to grow. Hence it is important that the root system should be in a state of continual growth, in order that it may be able to keep pace with the ever-increasing demands of the rest of the tree for water and nutrient salts. If the root system is not supplied with food from the leaves or stem, the growth of the rootlets will become slower, and finally stop altogether; absorption by the roots will then cease, and this will be followed by the death of the crown, or at least of the younger branches. Similar effects will be produced if the soil becomes waterlogged; for all parts of the tree absorb oxygen in the process of respiration, and if they are deprived of oxygen they cease to grow. In an ordinary soil the spaces between the soil particles are filled with air, and the roots obtain their necessary oxygen from that; but if the soil is waterlogged the interspaces are filled with water, and if that condition persists for too long a time, the roots are drowned. Drainage and forking benefit the plant by improving the aeration of the soil.

Transpiration. The water and nutrient salts absorbed by the root hairs pass into the wood in the middle of the root. Thence they proceed up the stem through the vessels of the sapwood and into the leaves *via* the vessels in the veins. There, much of the water is lost by transpiration—that is, it is given off in the form of vapour through the openings (*stomata*) in the surface of the leaf. This process is not simply evaporation from the leaf, but an operation regulated by the plant. The current of water from the root to the leaves is known as the transpiration current; it conveys crude food materials to the leaves and replaces the water lost by them.

Assimilation. The carbon dioxide of the atmosphere enters the leaves through their stomata, and is absorbed by the green tissue. There it is broken up and recombined with the water from the roots to form carbohydrate in the form of sugar. This process, which is known as assimilation, takes place only in green tissues and only in the light. Hence shading the leaves reduces the rate of manufacture. Other chemical actions between the water, the nitrates, and the sugars result in the formation of soluble nitrogenous compounds known as amides.

Movements of Food. It was formerly supposed that the first carbohydrate formed in the leaf was starch, but it is now known that starch is only the storage form. As soon as the sugars and amides are made they are conveyed away to other parts of the plant, but if the leaf is manufacturing sugar more rapidly than it can be transported, it stores it temporarily in the form of starch, which is reconverted into sugar and removed during the night. Starch and fat, which are insoluble, are storage forms of carbohydrate; when they have to be moved to other parts of the plants they are first of all transformed into a soluble form, *i.e.*, sugar.

The food — sugars and amides — manufactured by the leaves is conveyed along the veins to the branches and thence to all living parts of the plant. At the growing points it is consumed, being converted into new cells and new protoplasm. Its transference is effected by the sieve tubes of the bast; hence the current of elaborated food travels down the stem just outside the cambium, and as the sieve tubes run almost vertically down the stem, its descent occurs chiefly in a straight line parallel to the axis of the tree. Lateral movement of the food in the cortex is a much slower process, since it must take place by diffusion from cell to cell through the cell walls, whereas the longitudinal movement occurs along the continuous sieve tubes.

The Storage of Food.

Much of the food, however, is not immediately required by the plant, but is stored up for future use. When the current of elaborated food travels down the stem, some of the sugar is transferred radially inwards along the medullary rays, and is stored in the wood, where it is converted into starch. In the cortex also starch is stored, while, according to some botanists, nitrogenous materials may be stored in the sieve tubes. These stores constitute the reserve food of the tree. In all trees the wood is the chief reservoir of reserve food, and the medullary rays are paths by which the food enters and leaves it.

In temperate climates starch, or other reserve food, is being continually stored in the stems of deciduous trees until the time of leaf fall. Shortly after the leaves have fallen the starch in the stem is at a maximum. It undergoes several changes in the stem during the winter, and finally is converted into sugar and transported to the growing points in the spring. When the tree has produced new leaves and flowers the starch content of the stem is at a minimum. Since *Hevea* is a deciduous tree, similar changes must occur in it; the new leaves and flowers are produced at the expense of the starch reserve, and therefore the store must be at a minimum when the tree has just put forth its new foliage. How far this affects the total quantity in *Hevea* is not known; it is certain that the reserve must be diminished, and, arguing from known instances, it might happen that at certain times the stem does not contain any starch. This must be reckoned with in any attempt to estimate the amount of reserve food in the stem; for example, in judging whether a renewed bark can be re-tapped by ascertaining the amount of starch in it, as recommended by Fitting. But in any case, the renewal of the bark should be slower at the time when the tree produces new leaves or flowers, since it must draw on its reserve to form these, and there is therefore less available for the construction of new bark. If the latex is a food, this periodic renewal of the foliage might be expected to reduce the amount of it also. Ridley has attributed a poor yield, in one instance, to the fact that the trees were producing a heavy crop of seed, but the available facts negative the idea that the rubber is consumed in that way. Suggestions, however, have been made that the variations in the yield at certain seasons are not due to the rainfall, as is usually supposed, but to periodic changes in the amount of food in the trees. It is evident that further investigation on this point is needed, but at present it may be said that the known facts are against such a supposition.

Root Pressure. As already stated, the water in the soil is absorbed by the root hairs. These hairs are developed on the roots, a short distance behind the tip ; as the roots lengthen, the older hairs die off, so that there is never more than a short zone covered with them. Each hair is a cylindrical sac, lined with protoplasm and filled with a cell sap, which contains various salts, &c., in solution, the concentration of the cell sap being greater than that of the soil water. When a hair is in contact with the soil water, a process known as 'osmosis' is set up, by which both solutions commence to pass through the cell wall. The weaker solution passes through the wall the more rapidly. A very large amount of the soil water, with its dissolved salts, passes into the root hair, while only a minute quantity of the cell sap diffuses out. Therefore the cell sap becomes diluted and the pressure within the cell is increased. Similar reactions next occur between the root hair and the cells of the cortex, so that the water is passed on into the latter ; and, when the cortical cells are fully distended, the water is discharged into the wood of the root, whence it passes up the stem as the transpiration current. In this way the roots force water into the stem, the force being known as Root Pressure.

The substances which can be absorbed by the root hairs must be such as are able to pass, in solution, through the cell wall and its protoplasmic lining. Some substances cannot enter through the cell wall, while others which are of use to the plant are not allowed to pass out into the soil water. In this way the root may be said to select its food, but the selection is dependent upon the structure of the wall and the nature of the cell contents, and not on any consideration of the beneficial or harmful nature of the substances offered to it. If a substance is presented to the root in a form in which it can pass by osmosis through the cell wall, then the plant has no power to refuse it, even though it is highly injurious.

Root pressure varies at different periods of the growing season and also at different times of the day. It is affected by various external conditions, such as temperature, humidity, &c. In temperate climates it is most active in the spring, and is responsible for the bleeding which occurs when the stem of a young tree is cut down then. It is one of the chief factors which cause the ascent of water in plants.

As the water from the roots ascends the stem some of it is side-tracked by osmosis into the cortex, to replace any deficiency there. The remainder passes into the leaves, where a large proportion of it is given off in the form of water vapour, through the stomata. This latter process is known as transpiration. Transpiration varies according to external conditions; it is more active when the air is dry and hot than when it is moist and cold, and is greater in bright sunlight than in dull weather. During a sunny day, water is lost by transpiration faster than the roots can supply it; therefore the total amount of water in the plant is diminished, and if too much is lost the plant may wither. But during the night water is again forced into the plant, so that the tissues become turgid, and it revives. This is especially well shown by the common balsam in Ceylon, which becomes quite limp in full sunshine, but revives within an hour if shaded. This loss of water by transpiration during the day explains why the latex of *Hevea* is more concentrated in the evening than in the morning of sunny days.

Effect of Tapping.

It will be evident from the forgoing accounts of the physiology and structure of *Hevea* that the ordinary tapping cut, which removes a strip of bark almost down to the cambium, interrupts the downward flow of manufactured food in the cortex. But if the incision does not extend to the wood the upward current of water in the wood is unaffected. The extent to which the downward current is interfered with depends on the depth and length of the tapping cut; if the cut extends to the cambium it is completely interrupted over the whole length of the cut, and if the cut also extends all round the stem the tree is ringed or girdled. In the latter case the downward current of food is arrested at the upper edge of the cut, and the tissues below the ring, both the stem and the root, can continue growing only at the expense of the reserve food stored in the wood and cortex below the level of the cut. When this reserve food has been consumed, growth must cease, since food cannot travel from one part of the stem to another in the wood. Fortunately the quantity of reserve food in a young *Hevea* is very large; probably for that reason the tree has been able to survive some of the irrational tapping systems to which it has been subjected.

It follows that it is certainly detrimental to the tree to tap it all round the stem at the same time. This was pointed out by Parkin in 1899, even though he only contemplated tapping at weekly intervals by means of separate incisions, a method which would interrupt the food current for a very short time as compared with the present system of reopening the cut. It is important to remember that the food current is interrupted so long as tapping is continued, but that if tapping is stopped, the renewing bark is soon able to conduct some food across the cut provided that the latter has not penetrated to the wood. In this respect modern tapping differs from girdling, as will be explained subsequently.

Girdling. Fitting has described the effect of girdling *Hevea* saplings one year old, and about six feet high. They were girdled at about eighteen inches from the ground, a length of from 3 to 4 cm. of bark being removed. Their circumference at 18 inches at the beginning of the experiment was 3·5 to 4 cms. When examined 50-60 days later, the leaves were fresh and the crown had continued to grow, but the circumference of the stem above the girdle was 5·2 to 5·8 cms., while below it was the same as before. Starch was present in the tissues above the incision, except in the callus at the edge of the wound; but below the incision it was completely absent, and in this region reducing sugar was almost completely absent also. On one of the trees young branches had developed below the girdle, but even that contained no starch in that region. He repeated this experiment with plants two or three years old, with some variations which need not be considered here; when examined after three and a half months the tissues below the ring contained no starch, except in the innermost layers of the wood. While in the case of the slender yearling all starch disappeared from the stem after a period of only fifty-one days, the thicker three-year-old plant, after three and a half months, still contained starch in the innermost zones of its wood.

Sixteen trees at Henaratgoda were doubly ringed in September, 1907. These trees were said to be twelve years old, but they were badly grown, and had apparently suffered from close planting and the shade of surrounding trees. Their circumference at three feet varied from eighteen to thirty-six inches (average 25·9); they were in general 'drawn up,' with poorly developed crowns only slightly branched. A complete ring about one inch wide, extending down to the wood, was cut away at a height of three feet, and another similar ring at a height of four feet, leaving an isolated cylinder of cortex about a foot long. The stems were thus divided into three sections, completely distinct as far as their food supply was concerned; no section could obtain any food from the other two. The cuts were not protected in any way. Fifteen months later all these trees were still alive, and, except in two instances, they did not seem to have suffered very much. Seven of them had developed strong shoots below the lower cut, and most of these bore shoots above the upper cut and on the isolated cylinder also; one tree bore two shoots below the lower ring, two on the isolated cylinder, and three above the upper ring. In these cases the new shoots supply food to the separate parts of the stem, and therefore the isolated cylinder and the lower part of the stem obtain some food in addition to their reserve, although they are cut off from the crown; these trees were therefore more likely to survive than the others. Of these seven, both cuts were healed over, at least in part, in three cases; neither was healed in three cases; while in the remaining tree the lower cut only was healed, probably as a result of the growth of three shoots on the isolated cylinder. Of the remaining nine, which had not developed new shoots on the stem, neither cut was healed in four cases, both were healed completely or in part in three cases, while in the other two, only the upper cut was healed. In no case was there any noticeable difference in girth above and below the ringed part. The trees which showed most damage were twenty and thirty-two inches in girth respectively. The top of the former was dying, and the lower cut only was healed on one side; as it had developed three shoots on the isolated cylinder and two below the lower cut, it is probable that its state was due to the diversion of its water supply to the new branches, not to the starvation of the root. The top of the twenty-two-inch tree was dead and broken off: neither cut was healed, but seven new shoots had developed, so that it is probable that the cause of death was the same as in the former case, though as the relative times of the death of the top and the appearance of the shoots are not known, this is uncertain.

The above example shows that the ill effects of girdling may not be evident for a long time. Owing to the smallness of their crowns, these trees might be considered unfavourable for experiment, since they would contain less reserve food than trees of better growth; but, on the other hand, the small crown makes less demand on the roots for water, &c., and would therefore be likely to suffer less than a larger one. It may be noted that since a girdled *Hevea* tree does not die rapidly even if the wound is not healed over, it should be possible to renew the continuity of the bark by the process of bridge grafting, though the latter has not to my knowledge been proved successful in the tropics.

Renewal after Exposure of the Wood.

When a piece of *Hevea* bark and cortex, extending down to the wood, is excised, the cambium, if not removed by the cut, is killed by exposure, and therefore no creative tissue is left over the wound. Consequently, the wound can only be healed by the growth of new wood and cortex arising from the cambium at its edges. This new tissue emerges from the junction of the wood and the cortex and spreads over the exposed wood. At first it is a more or less flattened plate, and if the wound is small, or the external conditions favourable, the tissue from all sides may meet in the middle without any exceptional swelling; but in general it soon forms a swollen cushion surrounding the wound. This tissue is known as 'callus.' It differs from the normal tissue of the stem in several respects. In particular, its wood fibres are not long and straight, but short, distorted, and crooked, constituting what is called 'wound wood.' Further, the amount of wound wood produced is always greater than the amount of normal wood which would have been produced on the same area if it had not been injured.

The size of the wound which can be re-covered with new wood and bark is usually considerably over-estimated. On young *Hevea*, wounds heal rapidly and completely, but experimental wounds, two and a half inches square, on twenty-three-year-old trees have made very little progress towards a complete reconstruction of the bark in three years. Recovery is more rapid if the wound is kept damp, and the common mixture of cow dung and clay secures this requirement admirably. But large wounds on old trees will leave permanently exposed wood whatever the treatment. It has been stated that when the callus has ceased to grow further over the wood, it may be made to grow again by cutting it all round the edge, but though operations of this kind may be of use in horticulture it is doubtful whether they could find a place in what is more nearly forest management.

Other things being equal, the growth of callus is most rapid from the upper edge of a wound, since the current of food travels *down* the stem. If the wound is long and narrow and runs vertically down the stem, the growth naturally appears to have occurred chiefly from either side. When a tree is ringed, the growth from the upper edge of the wound is much greater than that from the lower edge, and if it is doubly ringed the upper ring heals over before the lower, provided that no green shoots are developed between the rings to interfere with the original distribution of reserve food. It may sometimes appear, even in the case of a horizontal wound, that the growth had occurred equally from either edge, but this can only happen when the wound is narrow, and short in comparison with the whole circumference. Apart from the question of convenience of collection, this more rapid healing from above is one of the reasons for tapping downwards.

An excellent illustration of the effects of tapping cuts which penetrate to the wood and completely interrupt the food supply was incidentally furnished by a new method of tapping. A vertical channel was cut from a height of six feet to the base of the tree. Across this, horizontal incisions down the wood were made with a broad chisel, one foot apart; at the next tapping new incisions were made, each one inch below the former, and this process was continued for twelve tappings. When the available cortex had been completely tapped, seventy-two short horizontal incisions had therefore been made, exactly under one another. A few months later the site of the upper incision was marked by a fairly large swelling, while smaller swellings marked the positions of the thirteenth, twenty-fifth, &c., *i.e.*, of the other incisions made on the first day. The intervening incisions showed only slight swellings. Remembering the course of the manufactured food in the tree, the explanation of these differences is fairly obvious. The food from the higher parts of the tree flowed to the uppermost cut and was prevented by it from descending further in that vertical line, hence a large amount of wound wood was produced. But each of the remaining cuts made on the first day was only able to supplement its supply from the wood by the food in the strip of bark, one foot in length, immediately above it, and therefore the knobs produced by them were smaller. Again, the remaining incisions were separated from the previous cuts by only one inch of bark, from which some of the reserve food had already been abstracted; they could, therefore, obtain little new building material except by a lateral movement in the cortex, which is extremely slow, or by a radial flow from the wood; therefore the amount of new tissue formed was small in comparison with that formed over the incisions of the first day.

Since the amount of wood formed over a wound is greater than would be produced under normal conditions, the position of a previous wound even when completely healed is usually marked by a swelling on the stem. Theoretically such swellings should be obliterated, or at least any abrupt deviation smoothed down, by the general growth of the whole stem; but in young *Hevea* this levelling-up process appears to be extremely slow, or rather, the effect of the wound in promoting an increased growth of wood persists for a long time. Every injury to the wood is reproduced by the new wood 'in relief,' owing to the excessive amount of wound wood. After a short time the cambium produces normal wood again instead of wound wood, but it would seem that, in young *Hevea* at least, it retains the abnormal activity which enabled it to fill up the wound, and thus continues to form more than the normal quantity of wood over the wounded spots. The following instances illustrated this.

Trees, nine inches in diameter, and about twelve years old, which had been tapped on the old system of separate V's, were cut down because their renewed bark was rough, under the impression that the roughness was due to 'canker.' On stripping off the bark the wood was found to be covered with raised V's with arms about four inches long, corresponding exactly in position with the old tapping cuts. The sharp-edged crests of the V-shaped ridges were elevated rather more than a quarter of an inch above the level of the surrounding wood. On cutting into the stem, black lines, showing where the old tapping cuts had penetrated into the wood, were found buried beneath about an inch of new wood. In this case it was estimated that the tapping had been done about four years previously, but though the cut was quite narrow (only a single incision) and the amount of new wood required to cover it only small, the cambium had continued to produce an abnormal quantity of wood during the whole of that time, with the result that all the old tapping cuts were shown 'in relief' on the stem.

Other stems, about four inches in diameter, were submitted for examination, because the normally smooth bark was interrupted by gnarled patches, usually circular and varying from one half to three inches in diameter, surrounded by a margin formed by the upturned edge of the normal bark. The smaller patches resembled branch scars, but their number, as many as thirty-six on a piece of stem one foot in length, showed that that interpretation was not correct. On removing the bark, the wood beneath the scars was found to be swollen, but quite sound, and on chiselling out the sound wood to a depth of half an inch, thin black patches about an inch in diameter were disclosed. These patches represented wounds made on the stem about nine months previously, which had been overgrown by a new layer of wood and bark, but the wood at that point was half as thick again as the wood produced in the same time in places where the stem had not been injured. Similar relief patterns may be seen on trees tapped by the pricker. If the renewed bark is stripped off when only a few months old a series of ripples or small elevations will be found, one for each time a tooth of the pricker has touched the cambium, If the pricking has been done carefully, this may be all; but in many cases each ripple develops into a point or thorn, so that the new wood is covered with more or less conical thorns, arranged regularly in rows. This is much more noticeable in those cases in which the tree has been tapped by the pricker only, than when both the knife and pricker have been used, probably because the thickness of cortex left in the former case protects the cambium and favours the production of wound wood.

The effect of cutting down to the wood was also illustrated by a recently-invented instrument known as a 'latex releaser,' which liberated the latex by a clean, continuous cut right through the cortex. In one experiment with this instrument, tapping was stopped after a breadth of three or four inches had been operated upon, and after a few months the tapped area was swollen out into a convex band, which resembled the circular projecting moulding (torus) seen on ornamental columns and lamp-posts.

In many cases this formation does not interfere seriously with re-tapping on the same area. The gradual swellings often seen on renewed bark, and the general convex outline equally common, are in most cases due to this. Generally the renewed bark is thinner over the wounded area. A single large wound, provided that it heals up, affords a better renewed surface than half a dozen small wounds on the same area, since the former presents one continuously convex surface of wood as opposed to the irregularly undulating surface of the latter, and therefore the coolie is less liable to cut into the wood during tapping. The separate thorns produced by the pricker (when it penetrates to the wood) clearly represent the worst type in this respect, since the thickness of the renewed bark over and between the pricker cuts may differ by a quarter of an inch, though externally it is quite even.

In general, however, if the tapped area is extensively wounded, it subsequently presents a series of exposed patches of wood, each surrounded by a swollen callus, and even if these patches are ultimately healed over, the surface remains irregularly swollen and covered with rough, cracked bark for many years. Such trees have frequently to be omitted from the tapping round, or tapped higher up the stem. It is therefore of the utmost importance that wounding the wood should be avoided as far as possible, in order to secure a smooth, tappable renewed bark. It may justly be said that this question is of more importance than that of any disease at present known to attack *Hevea*, for in some cases the renewed bark is so rough that rubber can only be obtained from it in the form of scrap. The duration of a rubber plantation under normal conditions depends upon the character of the renewed bark.

Renewal after Tapping.

When *Hevea* is tapped by excision of part of the cortex, and a layer of the latter is left intact over the cambium, the process of renewal differs from that which follows wounding. Since the whole of the cortex is not removed, some of the sieve tubes,

&c., are left untouched, and, provided that the remaining cortical tissue does not die back for other reasons, the transport of food across the tapped area is not necessarily stopped entirely. That this is so is evident from any tree which has been tapped by the half herring-bone system. In such a tree the food available for the formation of renewed bark is obtained (*a*) from the reserve in the wood and cortex, and (*b*) from the downward stream of recently-manufactured food. The first of these can be drawn on by each of the tapping cuts independently, but if the downward stream is arrested by the uppermost tapping cut, then none of it is available for the lower four or five cuts. Under those circumstances the renewal of the uppermost cut should be much more rapid than that of the lower, and the accumulation of food there should cause the formation of a large swelling. Indeed, the case would be exactly similar to that already quoted (p. 72). But since neither of these things happens, it must be concluded that ordinary tapping does not completely intercept the downward current of food, but that some is transported across the tapping cuts.

Fitting has recorded the distribution of reserve food in an eight-year-old tree, tapped by the half herring bone, with six cuts, about a foot apart, extending over one-quarter of the circumference of the stem. After daily tapping for 156 days, there was then no starch or reducing sugar in the pieces of bark (about 10 cms. wide) left between the tapping cuts, nor in the bark above the uppermost tapping cut for a distance of 5 cms. In this respect, therefore, all the cuts were alike, except that no starch was found in the bark within 15 cms. below the lowest tapping cut. Again, starch and reducing sugar were absent from a vertical zone, 5 cms. wide, on either side of the tapped area. In the wood starch was present everywhere, except beneath the incisions, and there it was absent only from the outermost zones to a depth of 1·5 to 2 cms. The renewing bark did not contain any starch. This examination shows that each renewing area draws on the store of food in the stem immediately behind it, and that the upper incision has, apparently, no advantage over the others. It may be noted that the absence of starch does not indicate that no food is being received in that region, but only that it is being consumed as fast as it is brought in.

The layer of exposed cortex overlying the cambium on the tapped area soon develops a phellogen, which cuts off a thin superficial sheet of dry brown bark and protects it from further injury. The cambium then proceeds to form new bark and wood over the tapped area in the normal manner; it does not form wound tissue unless the tapping cuts have penetrated to the wood. Hence the renewed cortex is smooth. Moreover, some of the sieve tubes were not severed, and these, with the new ones formed by the cambium, will be able to transport a constantly increasing quantity of food across the tapping cuts. The effect of ordinary tapping, therefore, as far as the downward food current is concerned, will be transient as compared with the effects of girdling.

Conditions of Rapid Renewal.

The advice has been given that, in order to secure a good renewal of cortex, the sun should be allowed to have access to the stems, and that for this purpose the trees should be lopped if necessary. While there may conceivably be instances in which lopping, and consequent exposure of the stem to full sunlight, might be advisable, as, for example, in severe attacks of canker, it can certainly be said that, as a general practice, it would defeat the object desired. Renewal of the cortex is favoured by a damp atmosphere; it is said to be more rapid in the Federated Malay States than in Ceylon, probably because the latter has more decided dry seasons. Moreover, the sudden access of sunlight would certainly cause the renewing cortex to split, and the same might be expected to happen to the original cortex, if the trees had previously been heavily shaded. Because of the damper atmosphere among closely planted *Hevea*, the renewal in such a plantation will be more rapid than if the trees had been widely planted. Here, however, another factor has to be considered, viz., the amount of food in the tree which is available for the formation of renewed cortex. The greater part of this food is stored in the tree before tapping begins, and the tree draws on this reserve to provide material for reconstruction. During tapping nothing is added to the stores behind the tapped areas; on the contrary, they are depleted. Now, the amount of food in a young *Hevea* is astonishingly large, and, whether the trees are widely or closely planted, it is quite sufficient to ensure the renewal of the cortex. Therefore, for the first renewal the two trees will be practically equal, as far as their food reserve is concerned, while the closer planted have the advantage of a damper atmosphere. The latter will therefore renew their cortex the more rapidly. In subsequent renewals, however, the advantage rests with the widely planted trees. When the closely planted trees grow up their crowns interfere, and ultimately become mere bunches of leaves at the top of a long stem. A crown of this kind cannot manufacture an adequate supply of reserve food, and therefore the bark renewal must be slower. The influence of a damper atmosphere cannot compensate for lack of food. Lopping the trees, of course, diminishes the rate of manufacture of food still more. *Hevea* interplanted with cacao is, as far as the tapping area is concerned, in a damper atmosphere than if *Hevea* had been planted throughout instead of cacao. Moreover, since it overtops the cacao, the factor of a diminished food supply through interference of the crowns of adjacent trees does not enter into consideration. Therefore the renewal of the cortex on such trees will be more rapid than on widely planted *Hevea* without cacao. But this advantage is not one that can be made use of, since the disease factor puts mixed *Hevea* and cacao planting quite out of court.

The subject of bark renewal has been discussed above, quite apart from any consideration of disease. If diseases are taken into account, the damper atmosphere of a closely planted area must be regarded as a decided disadvantage, as it has been proved in the case of cacao. The details given are sufficient to show that it is not advisable to lop *Hevea* with the express intention of allowing the sun to have access to the stem in order to favour renewal of the cortex.

Excision v. Incision.

The method of tapping now in vogue, *i.e.*, by reopening the same cut, is usually styled 'tapping by excision,' whereas the Brazilian method, tapping by single cuts, is termed 'tapping by incision.' But the distinction is more apparent than real. 'Incision, not excision,' sounds well, but, like many other epigrammatic phrases which have been inflicted upon the rubber world, it merely indicates a disregard of the elements of physiology. It is so plausible that it is not surprising that the idea has been generally adopted, and that the attention of inventors has been diverted to methods of 'incision.' The popular view was expressed at the Rubber Exhibition of 1908: 'It stands to reason that carefully made incisions must do less harm than the tapping system by excision.' Much, of course, depends on that 'carefully made,' but while it is possible to avoid touching the cambium with the knives used in 'excision,' it is much more difficult in 'incision,' where the thickness of the cortex is not evident to the tapper. 'Incisions' made in the East, and, according to all accounts, in Brazil also, usually penetrate to the wood.

The meanings attached to such phrases by the planter usually differ widely from those intended by the author. In the present instance there is a widespread belief among planters that, when the cortex is merely 'incised,' the two sides of the wound re-unite and no part of it is lost, and such an idea is strengthened by the published comparisons of wounds in a cortex with wounds in one's fingers. But, after the cortex has been cut, it converts a narrow zone on each face of the wound into dry brown bark. Therefore, if the tapper 'incises,' the tree 'excises' the cortex round the wound, and the amount excised round a dozen cuts will certainly not be less than the amount removed in a dozen reopenings of the same cut. There is no such thing as simple incision, as the phrase is usually understood.

It is further stated that the amount of sap obtained from the sieve tubes is reduced by 'incising' instead of 'excising,' though we have also the declaration that, in the usual method of tapping, the impurities derived from the sieve tubes are more abundant in the first tapping than in the later ones. These two statements are of course contradictory, and of the two the latter is the more probable. It would certainly be expected that the amount of cell sap, &c., mixed with the latex from half-a-dozen separate cuts would be greater than that obtained in six reopenings of the same cut.

As far as the effect on the tree is concerned, the Eastern method is certainly the least harmful. In the half herring bone, the uppermost cut interrupts the downward current of food over a certain area, and the remaining cuts, being vertically underneath it, do not add to that effect; but in the Brazilian method, in which half a dozen or more separate cuts are made round the tree, each cut exerts its own full effect. Moreover, experiments in the East would appear to show that scattered cuts cause a more rapid diminution in the yield of rubber than the herring-bone system, or even than such a drastic system as the abandoned full spiral: and they do not give a greater initial yield which would compensate for this falling off.

If the separate incisions extend to the wood, as they generally do, each one completely interrupts the downward current of food over its whole length, and the total interruption may be little short of a complete ringing. In this case each cut ultimately produces a swollen knob, and these are effective obstacles to future tapping. The last fact constituted one of the chief reasons why the method of separate cuts was abandoned in the East. In dealing with plantation trees, it is obviously necessary to employ some system by which the trees can be tapped repeatedly, and the Brazilian method, which induces the growth of large excrescences, does not fulfil this requirement.

The Pricker.

But the term 'incision' is applied, not only to the Brazilian method, but also to those systems in which latex is obtained by using the pricker. The rotating pricker generally employed in Ceylon makes marks three to four mm. long and seven to ten mm. apart when rolled across a hard surface. These figures approximately represent the marks made upon the wood of the stem when the teeth penetrate to the cambium; measured on the outer surface of the cortex the incisions will be longer and proportionately closer together. There are three sytems which involve the use of this instrument. In the original system the tree is tapped by the half herring-bone, and both the knife and the pricker are used; latex is first obtained by excising the cortex in the ordinary manner, and then on the same or the following day a further yield is obtained by running the pricker along the edge of the cut. Thus the knife and the pricker are used alternately to liberate the latex, and the method combines incision and excision. The advantage claimed for this method is that it liberates the latex which lies nearest to the cambium. A second system, which is generally employed in certain districts in Ceylon retains the form of the half herring-bone, but differs from the original system in that no latex is obtained with the knife; the stem is pared so that some of the outer non-laticiferous cortex is removed, and a row of incisions is then made with the pricker. In this case the paring secures a smooth surface and enables the pricker to penetrate more deeply, but all the latex is obtained by the pricker. By the first system the cortex which has been operated upon is removed, but by the second it is, or rather should be, left on the stem. The third system, which has recently been put on the market, differs from the other two in that the stem is pricked all round; the bark is scraped or pared to secure a smooth surface, and a ring of incisions is then made round the stem, followed by other similar rings in subsequent tappings. The third system agrees, therefore, with the second in leaving nearly the whole of the original cortex on the stem. To the non-botanist it appears self-evident that the last two systems must be less injurious than excision methods, but in reality the damage is usually much greater.

It is obvious that some of the objections to the Brazilian methods do not apply to the first two systems. In particular, the interruption of the downward current of food occurs only over a limited area, *i.e.*, over a strip of cortex the width of the half herring-bone. But the interruption is greater than in ordinary excision tapping because the pricker cuts, as a rule, extend to the wood. It is indeed possible to avoid damaging the wood in the second and third methods, though the average coolie does not find it so; but in the first method the pricker penetrates to the wood practically at every stroke.

As the pricker is at present constructed, it severs the food channels over one-third the length of the tapping cut at the first tapping. If the incisions made in subsequent tappings were exactly vertically beneath those of the first row, there would be no further interruption of the food current, but in actual working the relative positions of the incisions of the successive rows are quite fortuitous, and by the time half-a-dozen rows have been made, the current will be as completely interrupted as it would be by excising the cortex down to the wood. When the pricking extends all round the stem, then it practically girdles the tree after a few days' tapping, and the girdling persists until tapping ceases. The idea that pricking interferes less than cutting with the stream of food to the roots is quite erroneous; the interference is, as a rule, greater because the pricker cuts extend to the wood. There is, however, this advantage in favour of pricking as compared with excision extending to the wood, that the cambium, under the protection of the cortex, is able to heal up the wounds of the former more rapidly than those of the latter. Obviously this advantage is not shared by the first method. It has been argued that the statement that the pricker, as employed in the third system referred to, rings the stem is incorrect, because small trees, which have been tapped by the pricker only, show a greater increase in girth than those of the same age which have not been tapped. But it must be remembered that the tree is ringed only so long as tapping is in progress, and that the continuity of the cortex is re-established shortly after tapping ceases. The increase in girth begins only after tapping has been stopped, and it is in a great measure the result of wounding the wood.

In the first system of pricking, the amount of cortex excised is equal to that which would be excised if the knife alone were used, but in the second or third system the amount excised is only that which dies round the pricker cuts, and is evidently less. On the other hand, it is possible to make twenty cuts to the inch with a knife, whereas only about eight rows can be made with the pricker; therefore the area operated upon in the same time in the latter case must be greater. If the rows of pricker cuts are too close together, the cortex is divided up into such small pieces that it frequently dies in large patches, up to a foot in diameter.

The object of the pricker is admittedly to obtain the latex which lies nearest to the cambium without exposing the latter. But if there is any truth whatever in the suggestions that what is first formed in the latex tubes is not rubber, and that the rubber globules take some time to mature, it is clear that this latex should not be extracted. For it is the latex most recently manufactured, and on the above suppositions it is 'immature,' but would yield good rubber if left long enough in the tree. It should be impossible for any upholder of the immature rubber theory to advocate the use of the pricker.

The renewed bark exhibits the effects of the pricker less after the application of the original system than after the second or third, and it has already been stated that it is usually in the second and third that large thorns are formed when the pricker penetrates into the wood, probably because the remnants of the cortex protect the wounds from desiccation, and thus favour the production of wound wood. The main objections to the use of the pricker have always been based upon the character of the renewed bark. Immediately after the pricking, the renewing cortex is pitted, but this, the only outwardly visible effect, is not of much importance, since the pitted layer will be converted into non-laticiferous tissue before the bark is retapped. The most serious damage is internal, and can only be detected with the microscope.

Professor Fitting has shown that wherever the teeth of the pricker penetrate to the neighbourhood of the cambium, the latter subsequently produces, not normal cortex containing latex tubes, but groups of hard, thick-walled cells, known as stone cells, similar to the groups of hard cells which are found in the flesh of a pear, and which give the latter a gritty feel. Stone cells are quite common in the normal cortex of *Hevea*, and apparently they are more prevalent in renewed than in original bark, but the groups formed under the pricker cuts are readily distinguished, from those which occur naturally, by their position in the cortex. In general, all these groups are ellipsoidal, but while the long axes of the ordinary groups are usually vertical, those of the masses resulting from the pricker cuts are perpendicular to the cambium. It follows from Fitting's discovery that the renewed cortex after the use of the pricker is less laticiferous than after the knife alone, since the latex tubes occur only between the pricker cuts.

But the foregoing result is less formidable than it appears at first sight. The formation of these abnormal groups of stone cells is due in some way or other to the shock given by the pricker to the cambium, but after some time this effect wears off and the cambium reverts to its normal condition. Examination of a piece of pricked cortex, six months after pricking, showed that the abnormal groups of stone cells were still in contact with the cambium, though it was evident from their shape that their formation was about to cease. This particular piece of cortex had been pricked but not pared, and therefore it should show the effect of the pricker to the fullest extent. A second sample was taken from a tree that had been pricked and excised, about twelve months after the operation. The thickness of the renewed cortex was 4 mm. at the point beneath the pricker marks, and the abnormal groups of stone cells extended from the exterior to a distance of 1·75 mm. from the cambium. Nearly one-half of the renewed cortex was of normal structure, and it would therefore appear that the formation of abnormal stone cells ceased about six months after pricking. In all probability it ceased earlier than this, for as the following example indicates, the renewing cortex does not increase in thickness at a uniform rate. The increase is greatest immediately after tapping; afterwards it slows down until it ultimately approximates to that of the untapped cortex. A third sample was taken from a tree which had been similarly pricked and excised three years previously. The thickness of the renewed cortex was 5½ mm., and the groups of stone cells extended from the exterior to a depth of 2½ mm. from the cambium. Here again nearly half the cortex was of normal structure. It should be noted that all these measurements were made on cortex preserved in alcohol, and that they do not include the outer brown bark.

It may be concluded from the above that the effect of the pricker on the cambium passes off after about six months, but the groups of stone cells persist in the outer half of the cortex after three years from pricking. As almost the whole of the latex is obtained from the innermost two millimetres of the cortex, it is evident that the stone cells will not have much effect on the flow of latex after three years. Therefore, as far as is known at present, this non-laticiferous character of the renewed bark and the obstruction to the flow of latex by the stone cells need not be taken into account in a four-year system of tapping.

The renewed bark may, however, exhibit another effect of the pricker which is more serious than that referred to in the foregoing paragraphs. It has already been stated that when wounds extend to the wood, a large quantity of wound wood is produced, and this may give rise to a correspondingly large excrescence on the stem. In that case the wound wood is continuous with the wood of the stem. Such excrescences ultimately become more or less merged into the stem, and do not offer much obstacle to retapping. But there is another type of excrescence, or burr, of totally different origin. These are the result of the formation of a nodule of wood in the cortex quite free from the main wood of the stem. At first they are only small, but as each nodule is surrounded by a cambium of its own, it increases in size until it attains a diameter of several inches. The burrs which contain these nodules may be hemispherical, but more usually they are vertically elongated; and they project abruptly from the stem, so that the tapping cannot be carried across them. As they increase in size, the cortex overlying them cracks;

and since several arise close together, as a rule, the stem is covered with large irregular outgrowths and patches of dry bark. It is quite impossible to tap a stem of this kind.

Further details of these burrs will be given later. The important point at present is that though these nodules do sometimes occur on trees that have never been tapped, they certainly appear to be more prevalent on trees which have been tapped by the pricker. Numbers of trees bearing these burrs have been examined during the last five years, and specimens of trees rendered untappable by them are constantly being sent in for report, and in nearly every case the pricker has been used. The view that the pricker favoured the production of these nodules was put forward in 1906, and though the theory was not then accepted, the subsequent condition of the trees tapped by it led to its abandonment on one of the largest estates in Ceylon, where it had been extensively used. The nucleus of each nodule is either a small group of dead bark cells or a group of stone cells. It was at first thought that these might have been pushed into the cortex by the teeth of the pricker, but Fitting's researches suggest that in the case of the stone cells the nucleus may be provided by the abnormal groups of them which are produced by the cambium after pricking.

At the present day the only valid objection to the use of the pricker is that which caused its abandonment in 1907, viz., that it favours the production of nodules in the cortex, and consequently induces an untappable renewed bark. In 1909 it was brought into use again in a new incision system, which was given an exhaustive trial in Ceylon on a large number of estates and ultimately rejected. On this occasion the pricker was discarded because the system was too expensive and did not produce the yield anticipated. As there is every reason to expect that other pricking systems will be advocated, it may be as well to state that the full effect of the pricker on the renewing bark cannot be expected to be visible, as a rule, in less than two years.

It is stated that in certain districts a good flow of latex cannot be obtained with the knife, and therefore the pricker must be used. The following experiment may afford a reason for this. A piece of cortex was shaved off the stem of a *Hevea* at one cut with a large knife, thus suddenly laying bare an oval patch of wood. In addition to the flow of latex from the cortex, there was also a flow of clear sap from the outer layers of the wood. The experiment was done in the afternoon, and on a fine day, though in the rainy season. It would seem probable from this, that where the pricker must be used the flow of latex is assisted by the flow of sap from the wood. In that case it might be said that the district was wet enough to grow *Hevea*, but not wet enough to admit of its being tapped in the ordinary way.

Scraping. In the early days of plantation rubber tapping, when the latex was allowed to run down the stem into cups placed at the base, it was recommended that the tree should be carefully and lightly shaved so as to form a perfectly smooth surface. At the present day this shaving is still carried out in some instances, though there does not appear to be any reason for it now that the latex is conducted to the collecting cup by a vertical channel. If the outer brown bark is scraped away, and the underlying cortex exposed, the latter soon cuts off another layer of bark, and if the cortex was cut during the operation of scraping, this new layer of bark is all the thicker and is developed more rapidly. Therefore, if scraping must be done it should be done as lightly as possible, and not long before the scraped area is tapped, otherwise the advantage sought is quite lost. The new brown bark is, of course, formed at the expense of the cortex, and the latter therefore is thinner after its development; but as the outer layers of the cortex which are converted into bark would not have yielded latex, nothing is lost by this conversion, except perhaps a little reserve food The exposed cortex is liable to crack longitudinally before a new protective layer of bark has been developed. If the cortex is shaved too deeply, it often dies down to the cambium in patches up to about four inches in diameter. In some instances this is due to the attacks of the canker fungus, *Phytophthora Faberi*, but in others the characteristic symptoms of this disease are wanting, and the death of the cortex would seem to be due to exposure to sunlight, &c., rather than to the action of fungi. When the patch of dead cortex separates from the wood, the rupture often extends along the cambium into the surrounding healthy tissue. Probably the daily expansion and contraction of the stem may assist in causing this extension. Latex then flows *from the healthy tissue* into the space between the dead cortex and the wood, and coagulates there, usually in the form of a plano-convex circular pad. If the cortex has cracked, some of the latex exudes and coagulates on the exterior, but in the majority of cases the rubber pads which are formed after the bark has been scraped are entirely covered by the dead cortex and are not discovered until the bark is tapped.

These pads between the wood and the cortex were especially common in Ceylon in 1909, when the Northway tapping system was under trial. This system necessitated the shaving of the lower part of the stem, and in many cases too much was shaved off. The pricker was blamed for causing them, but in all the cases examined it was evident that they had been present before the cortex was pricked. They were found behind untapped cortex; and where they occurred behind tapped cortex, they bore cuts into which pieces of bark had been pushed by the pricker. Several explanations of these pads were offered in the local press, but all ignored the fact there must be a space between the wood and the cortex before the latex can collect there; it is impossible that it should flow into solid tissue.

Exudations of Latex.

Exudations of latex from untapped stems of *Hevea* are not uncommon, but the cause of them is not yet known. It may be laid down, as a general rule, that only healthy cortex can yield latex; if the cortex is attacked by fungi, the latex coagulates in the latex tubes, at least in all the fungus diseases of *Hevea* known at the present day. Therefore any exudation of latex indicates that healthy cortex has been ruptured or cut. The prevalent idea is that the cortex bursts, but why it should burst is not clear. The phenomenon is well known in Brazil; at the Rubber Exhibition of 1908 it was stated that the tree sometimes burst and the latex flowed from it, in which case a big lump of coarse rubber was found at the wound. Specimens of such lumps have been sent to Ceylon from Bolivia, under the impression that they were identical with the rubber pads referred to above.

This phenomenon is not confined to *Hevea*. Spontaneous exudation of latex has been recorded in *Manihot piauhyensis*, and it has been observed in *Manihot dichotoma*, at Gangaruwa, after heavy rains. Riviere (*Journal d'Agriculture Tropicale*, December 31, 1909) records the same for *Ficus macrophylla*, and attributes it to excessive pressure of the latex; he states that on strong branches the cracks reach a length of 40 centimetres, the wound subsequently enlarging to a breadth of 5 or 6 centimetres, but that it does not occur on a regularly tapped tree. In *Hevea* it seems to be more common on the smaller branches, small cracks occurring from which the latex runs down and coagulates in black streaks. Whether the pressure of the latex inside the tree can burst the cortex must at present be considered doubtful. So far as the stems and branches which have reached the secondary stage are concerned, it would not appear possible, but in green stems it might happen. The behaviour of *Manihot dichotoma* after heavy rains suggests that it does so happen in that species.

One frequently finds that the exudation of latex is regarded as a characteristic symptom of this or the other disease. For example, it has been described as the first symptom of 'dieback,' and one of the characters of 'pink disease.' That coagulated rubber may be found on bark attacked by *Corticium salmonicolor* is undeniable, but its presence there is a secondary matter, and it does not occur until the tree has been attacked for some time. There are two ways, at least, in which such exudation can occur. When bark is attacked by pink disease it dries up, cracks, and splits away from the wood. These cracks may extend into the surrounding healthy tissue, and the latex then exudes from the latter. When the fungus spreads further it involves the bark from which the latex issued, and therefore the strands of rubber are found on diseased bark. But the latex issued from this bark before it was attacked. Again, in the early stages of pink disease and canker, the disease may affect only the outer half of the cortex, the part next the cambium being as yet unattacked. If this diseased bark is bored by beetles, the latter may draw latex from the inner sound tissue. This especially occurs if they bore into the bark when the latex will not flow; they then penetrate into the sound cortex without drawing latex at the time, but the latex exudes from the hole after the tree has absorbed more moisture. Latex may also exude from tapped trees when the renewing bark cracks owing to exposure to wind and sun.

'Hide-bound' Trees.

It has been suggested that when young *Hevea* trees become 'hide-bound'—as they sometimes do when grown at high elevations or on poor soil—they would be improved by rubbing off the outer layer of dead bark. It is not probable that such a treatment would have any effect, since the layer in question is thin and usually cracked, and does not exert any pressure on the tissues underneath. There seems to be an idea current that rubbing off the outer brown bark will 'irritate' a tree, and cause it to increase in girth more rapidly. But, apart from the fact that irritation of this description is unknown in plants (except, perhaps, in certain tendrils), it will readily be seen that such a process would not affect the cambium, which alone is responsible for the growth in thickness. The only result of rubbing off the layer of dead bark would be that the tree would produce another layer at the expense of the laticiferous tissue, as is evident when the tree is scraped. A similar treatment was advocated about 150 years ago, when it was recommended that tree trunks should be washed and scrubbed in the spring in order to promote an increase in girth; the method received many testimonials of its success—as is usual with all new ideas in agriculture—but the absence of any such practice nowadays is, perhaps, the best indication of its efficiency.

Root Pruning. Root pruning of *Hevea* has been recommended, apparently under the belief that what was good for oranges and coffee must be good for *Hevea.* The main object of root pruning was to procure a large crop of fruit. A striking example of this is given in the *Journal of the Royal Horticultural Society* for March, 1909. A horizontal pear-tree, 12 years old, which had never borne any fruit, was root-pruned on one side. Two years later that side of the tree was covered with fruit, but there was none on the other. If *Hevea brasiliensis* were grown for seed only, root pruning might be advisable; but if a good growth of foliage is desired root pruning should be avoided. Root pruning is also involved in the recommendation that green manures, &c., should be trenched in round the tree at a distance of one foot for every year of the tree's age, since the growth of the lateral roots is in general much more rapid than that.

Forking. The present system of manuring and forking would appear to be open to reconsideration, especially the method of manuring in circles round the tree. Absorption of the manures occurs only through the fine white rootlets, and in a six-year-old plantation these will seldom be found within a distance of six feet from the tree. If the trees are planted fifteen feet apart their roots will have crossed by that time, and the manures applied may possibly be taken up by the trees in the next row but one. It would seem that they would be equally available if they were applied in lines between the rows—which would entail less labour—or broadcast, which might be done with less injury to the roots, and would reach a larger number of rootlets. The present system of forking round the tree often entails enormous damage to the roots, especially in closely planted areas. Whether this will have any markedly injurious effect on the tree remains to be seen; but it is certainly open to question whether it is advisable to apply horticultural methods to the cultivation of *Hevea*.

Thumb-nail Pruning.

When the stem of a *Hevea* forks at a distance of six to ten feet from the ground it is usually thicker than a straight-stemmed tree of the same age. Hence it was proposed that young trees should have their terminal bud removed when they had attained that height, in order to cause them to fork; in this way the lower part of the trunk might be expected to attain what is generally regarded as a tappable size sooner than if the tree were allowed to develop a straight main stem. While some increase is to be expected if this plan is adopted, the difference obtained from measurements of the Henaratgoda trees, viz., a gain of one inch per year for thirty years, is somewhat misleading, since it ignores the fact that the thickest forked trees are in more open situations than the others. When the tree first forks it has practically twice the leaf area of the straight-stemmed tree, but when the two stems begin to branch, their branches interfere with one another, and the ultimate size of the crown may not exceed that of the straight-stemmed tree. Therefore the maximum effect should be observed when the tree is small, and it should diminish annually.

In some cases the desired branching was not attained, since, as often happens when trees are topped, one shoot developed faster than the others and grew straight up. In other cases, numerous shoots developed, and several of them had to be removed; this is especially likely to happen if the tree is pollarded, *i.e.*, cut across low down the stem, instead of being 'thumbnail-pruned,' *i.e.*, having only its terminal bud removed.

M. A. de Ryckman (*Journ. d'Agriculture Tropicale*, January 1909), while recording a somewhat unsuccessful attempt to induce branching by this method, has raised the objection that if successful it produces an undue development of the crown, and therefore upsets the balance between the crown and the roots, the idea apparently being that the roots are developed only to such an extent that they can supply water, &c., to the normal crown, and that if the leaf area is abnormally increased they will not be able to keep up a proper supply. There does not appear to be much danger of that. But he also objects that the forked stem is more liable than the straight stem to the attacks of 'pink disease,' and for that reason thumbnail pruning should be avoided. This second objection is a sound one, and where 'pink disease' is prevalent, forking should not be encouraged. However, the method was never widely adopted, and experience has shown that there is another drawback which renders it undesirable. When trees have been made to fork in this way, their branches break off even in a moderate wind, and, generally, not only does the branch break, but part of the main stem is torn away with it. In some cases the main stem is split almost to the base, and the tree is lost altogether. The structure of the fork in induced forking appears to differ from that in natural forking

CHAPTER V.

Tapping Systems and their Effects on the Tree.

The first tappings of *Hevea brasiliensis* in the East imitated more or less Brazilian methods, separate oblique incisions or small V's being employed, and new cuts made for each day's tapping. Willis, in 1897, tapped trees at Henaratgoda by vertical rows of small V's, the arm of the V being only three-quarters of an inch in length; these rows were made at intervals of about ten inches round the tree, and each contained six V's, one foot apart. On a tree thirty inches in girth, four rows, each containing six V's, were advised. The trees were tapped at intervals of a week, new rows of V's being made between the old. Parkin, in 1898-99, tapped by both separate V's and separate oblique cuts, but the incisions of one day's tapping were made in horizontal rows round the trees, not in vertical lines; in one experiment five V's were made at the base of the tree round half the circumference, and another five were made round the opposite half at a height of six feet, the V's in each case being about six inches apart; the trees were tapped at intervals of three to seven days, the V's in the successive rows being four to six inches from those of the previous row and alternating with them. But prior to this, the separate incision method had been abandoned in Singapore, Penang, &c., in favour of the method of reopening the same cut; Curtis, in his report of the Penang Botanic Garden for 1897, refers to the latter method, and, soon after this time, it was introduced into the Kalutara district, Ceylon, from the Federated Malay States. Parkin appears to have been acquainted with this new method, since in his circular, issued in 1899, he wrote: 'The old wound may be renewed with a knife several times, and a good yield obtained thus, but the final result is liable to be a very ugly wound in the tree, which may lead to decay or other injury.'

Experience has shown that (1) the wounds of the old method are worse than those of the new; (2) the renewal over the separate cuts takes the form of large swellings, so that the tree cannot be tapped on any regular system; (3) that the yield falls off more rapidly in the old method. With regard to the latter point, the following facts may be noted. In Willis's experiment of 1897 (with separate incisions), the yield of rubber at the second tapping was 200 per cent. of that at the first; thence it fell to 133 per cent. at the third tapping, 110 per cent. at the fourth, 92 per cent. at the fifth, and 71 per cent. at the sixth. But in the corresponding experiments carried out at Henaratgoda by Bamber and Lock in 1908, when the trees were tapped by large V's extending half-way round the tree, and the wounds were renewed at weekly intervals, the yields at the second to sixth tappings were 148, 165, 170, 151, 124 per cent. of that at the first; further, the yield did not fall below that of the first tapping until the thirty-sixth tapping. The *actual* yields of these two sets of trees are not comparable, because the average girth of those tapped in 1908 was fifty per cent. greater than the average girth of those tapped in 1897, but the percentage calculation shows the difference in the variation of the yield, which is probably due to the system employed.

As there is some probability that the difference between relative yields in the two experiments quoted above may be due to differences in the weather, the following instance is also given. Wright tapped eight trees, twenty-nine years old, at Peradeniya, in 1905, four of them by the full spiral, four curves on each tree, and the other four by large V's. Each tree of the latter four bore twelve V's, with arms about a foot in length, arranged in four vertical rows of three. The V cuts were reopened at each tapping, so the method was not really that of separate incisions. But after tapping for ten weeks, every day or every alternate day, the flow of latex from the latter group was too small to warrant further tapping, though there was still plenty of space between the adjacent V's; the yield at the end of five weeks (15 tappings) was 2 lbs. per tree, and at the end of twelve weeks (50 tappings) 2 lbs. 13 ozs. On the other hand, the yield per tree from the group tapped by the full spiral was 1 lb. 9 ozs. at the end of nineteen days (15 tappings), 2 lbs. 10 ozs. per tree at the end of five weeks (23 tappings), and 4 lbs. 3 ozs. at the end of twelve weeks (57 tappings);

and the tapping was continued for thirty-five weeks (150 tappings), the total yield per tree being 6 lbs. 13 ozs. It will be seen that the tappings of the two groups were irregularly distributed, but as they were done in the same season they serve to show that the greater number of wounds interferes more with the flow of latex, and causes a more rapid diminution in the yield. That the yield of the V tappings was greater than that of the spiral in the first fifteen tappings is due to the fact that the latter were tapped almost every day, while the former were tapped every alternate day.

There are no records which would enable a comparison to be made between the yields obtainable in Brazil by the method of separate incisions and those obtained in the East by the 'reopened incision' system. The various accounts which have been published of Brazilian methods seldom afford the necessary data, and, when they do enter into details, the details of any one account are usually mutually contradictory. If, for example, we are told that the tapping season lasts 120 days, that each man taps 100 trees per day, that a tree is only tapped three times, that some trees yield 8 lbs. per season, and that the total return per man is 4 or 5 cwt., there is evidently something wrong with the figures, even if we suppose that the tapper only works every other day.

Renewed Incision Systems.

The method of reopening the old cut had been introduced before the beginning of the present century, but it is only within the last three years that there has been any indication of a general agreement as to the best system of applying it. The earlier systems were V cuts, or oblique incisions vertically under one another, each V or each incision requiring a separate cup. These were followed by the full or the half herring-bone, the cuts of the previous systems being united by a vertical channel. Such methods were usually employed conservatively, the tapped area extending over not more than half the circumference, and the tapping being interrupted by lengthy resting periods. In 1905-6, the full spiral and half spiral were introduced; by these methods the whole of the cortex of the lower six feet of the trunk was removed in a year in daily tapping. It was soon realised that these last-named systems were too drastic, and that the yield did not exceed that obtainable by other methods; consequently they were soon abandoned, except on a few estates. At the present time the system most generally employed is the half herring-bone, extending over one-quarter or one-half of the tree.

The Full and Half Spirals.

The full spiral system consisted of oblique cuts at an angle of about forty-five degrees, extending from a height of about six feet down to the base of the tree. The number of cuts depended upon the girth of the tree, there being usually one for every ten or twelve inches. All the cuts began at the same height, and each one was carried round the tree to the base. The number of times each cut encircled the tree also depended upon the girth; on a tree five feet in circumference each cut went rather more than once round; on a tree two feet in circumference it would make three complete turns.

The most glaring fault of this system was that the tree was tapped all round at the same time; and as the lateral movement of food in the cortex is slow, the roots were cut off from all supply of food from the crown, as long as tapping lasted, except for the small quantity, if any, which might pass through the layer of cortex left over the cambium. Where pricking was combined with excision, the food supply was undoubtedly interrupted entirely. The roots could therefore only develop at the expense of the food previously stored in them. Fitting investigated the distribution of starch and sugar in a *Hevea* stem tapped by the full spiral, and came to the conclusion that full spiral tapping produced the same effect as girdling the tree; in his experiment only one spiral curve, encirling the tree one and a half times, had been cut, and tapping had been continued ninety-six days, during which time about five inches of cortex had been removed along the cut. The tree was eight years old, and measured twenty inches in girth at about four feet; it was therefore small, but on the other hand the tapping was light, for spiral tapping, since there was only one spiral.

When young trees are regularly tapped by the full spiral, they show the effects of 'overtapping' before all the cortex has been removed. These effects are first exhibited by the crown. The green shoots usually die back, because of the starvation of the roots and the consequent cessation of their growth. Old trees do not suffer to the same extent, because they have a greater reserve of food. The specimen trees tapped at Henaratgoda and Peradeniya were 20–30 years old, and were being tapped for the first time; hence it is scarcely surprising that some of those who inspected them formed an erroneous opinion of the possibilities of the system. It cannot, however, be said that it was ever a favourite system in Ceylon, and, in general, it was soon abandoned; though as late as 1909 advocates could be found who declared that it was the best system they had tried, and gave the biggest yields with the least damage to the tree. But even these have since abandoned it.

One of the Peradeniya trees tapped by the full spiral in 1905-6 was examined in 1910, four years after the tapping ceased. It had been tapped by four spiral curves, each extending rather more than once round the tree. The circumference of the tree at three feet was sixty inches. It was tapped 150 times in a period of thirty-five weeks, and all the cortex was removed except four strips, varying in width from 1½ to 3½ inches. In September 1910 starch was present in the wood and the cortex over the old tapping cuts at the base of the stem. This tree was therefore retappable after four years. In all probability it was retappable much earlier, but unfortunately no previous examination was made. The case therefore merely supplies the knowledge that the tree can be safely retapped four years after it has been tapped by the most drastic system yet invented. At the same time, it must be remembered that this tree was tapped for the first time when it was twenty-nine years old.

The half-spiral system apparently consisted of oblique cuts similar to those of the full spiral, but not extending to the base of the tree. It appears to have been a kind of imperfect full spiral, to be adopted when, by reason of some irregularity in the cortex, the cut could not be carried down to the base. It was not a distinct system, but a modification which circumstances might compel the user of the full spiral to adopt. The available photographs of trees tapped by the half-spiral are in some cases clearly full-spiral tapping with extra cups affixed 'for that occasion only.'

The Half and Full Herring-bone.

The full herring-bone consists of four or five V's vertically under one another, with their apices united by a vertical channel. In this form it was found that the piece of original cortex left within the V sometimes died back, and therefore it was modified so that the cuts on one side of the vertical channel alternated with those on the other. If the vertical channel is not too deep, this modification has also the advantage that latex is better able to flow into the bridges of cortex between the tapping cuts, at least during the earlier tappings. The chief objection to this second form of the herring-bone is that the lowest cut on one side of it must be higher than that on the other by half the distance between the original cuts, and therefore part of the bark at the base of the tree is left untapped. It does not, however, follow that no latex has been drawn from that piece of bark, and it may therefore be doubted whether this objection is worth much. In some circles the original herring-bone system is now known as 'adjacent quarters,' while the modification of it is called the full herring-bone : hence the opinion has been expressed that the full herring-bone is inferior to the system of adjacent quarters because of the pieces untapped.

One result of the full herring-bone and adjacent quarters systems is an increased growth along the line of the vertical channel. The tree 'loses its shape,' or becomes 'torpedo shaped.' This has been explained by the supposition that the growth of the stem is diminished over the tapped surface except along the vertical channel, and therefore the stem bulges out along the latter. But, as a rule, there is no marked diminution of growth over the tapped area, especially on young trees ; in general, the increase over the tapped surface is greater than over the untapped. Further, the above explanation supposes that the cortex is left intact along the vertical channel. The more probable explanation would appear to be exactly the opposite of this. It will generally be found that the coolie cuts deeper near the channel than elsewhere, and, though it may be cut quite shallow at first, it often extends down to the wood, at least in places, as the tapping proceeds downwards. The extra growth along the channel, which throws the tree out of shape, is usually due to the development of wound wood along that line, and hence it projects beyond the general curve of the more carefully tapped surface. It is stated that the vertical channel need not be cut so deep as to interrupt the lateral movement of food ; in actual practice it generally does.

The full, or half, herring-bone uses up the food stored in the cortex over the whole tapped area, together with that in a zone two or three inches wide above and at the sides. If a rectangle be drawn surrounding the herring-bone, with its upper side three inches above the uppermost V, and its vertical sides three inches from the extremities of the arms, no starch will be found in the cortex remaining on that area towards the end of the tapping period in daily tapping. If the tapping interval is longer, the width of the rectangle may be less. In the wood starch is removed from those regions immediately behind the tapping cuts, so that, at the end of the tapping period, it will be absent from the wood all over the tapped area to a depth which, again, may be dependent upon the interval between the tappings. But the latex is drawn from a greater area of cortex than that which is deprived of starch. It is evident that the greater part of the food consumed in the renewal of the cortex is derived from the wood; but some is drawn laterally from the surrounding cortex, and therefore the flow of food to the roots is interrupted over a breadth rather greater than the width of the herring-bone.

Trees have been tapped at the same time by two herring-bones, or two half herring-bones, which together completely covered the whole circumference. Such methods have now been generally abandoned in favour of the system, first advocated by Ridley and since emphasised by Fitting, of confining each herring-bone to one-quarter of the circumference. But there are still many estates on which it is extended over one-half the circumference; the system of adjacent quarters, for example, is a full herring-bone over half the circumference. Whether each quarter should be tapped by a full herring-bone or half herring-bone is more or less a matter of opinion. The latter should require less labour, but the former has the advantage of allowing more food to move laterally from the surrounding cortex into the spaces between cuts, since it can move obliquely downwards on both sides. It may, however, be questioned whether this advantage is very considerable. It is claimed for the half herring-bone that it allows one vertical channel to be used for two quarters; but unless the adjacent quarters are tapped at the same time it will require to be reopened. In any case, there are strong reasons in favour of making a new channel for each quarter. The half herring-bone is the system most in favour at the present time.

If only one-quarter of the tree is tapped by, say, the half herring-bone, the current of food is interrupted over rather more than a quarter of the circumference. When that quarter has been completely tapped food will be absent from a zone of cortex two or three inches wide round the tapped area, and the latex will contain less than the original percentage of rubber over a still greater zone. Both the adjacent quarters are therefore deficient in reserve food and rubber, at least along one side. If opposite quarters are tapped at the same time, then this deficiency affects both the intermediate quarters from both sides. In the latter case it is certainly undesirable to tap the intermediate quarters immediately. Though we have no information as to the yield of rubber under such circumstances, it must be much smaller than from the first two quarters tapped, while there is no doubt whatever about the diminished amount of food in the cortex.

Fitting recommends that the first quarter should be tapped for two or three months, then rested for one or two months, and finally finished in two or three months more. After that he considers that the tree should be rested for five or six months, so that the second period begins a year later than the first. In the second year these operations should be repeated on the opposite quarter, and in the third and fourth years the intermediate quarters should be similarly tapped. As the strip tapped in the third year adjoins that tapped in the previous year, he considers that it might be advisable to leave a narrow, vertical strip of untapped bark between them, in spite of the fact that the tree has been rested for six months. If it is desired to tap both sides of the tree at the same time—for the reason that two short incisions at some distance apart yield more rubber than one continuous incision equal to their combined length —he advises that the tree should be divided into eighths instead of quarters.

At the present time the probability that the system described above will be adopted is very slight. Tapping the whole circumference is still in vogue on some estates, and tapping half the circumference at the same time is quite common. Moreover, 'resting periods' are tabooed, and it has even been declared that there is no strong botanical reason in favour of them. Where only one quarter is tapped at a time tapping is usually continuous, and one quarter follows the other without any break.

The following plan is already employed on some estates, and may be recommended as a fairly conservative system. One quarter is tapped by the half herring-bone every other day for a year, all the cortex of that quarter being operated upon. In the following year the opposite quarter is similarly tapped, and the intermediate quarters are tapped in the third and fourth years. The alternate day tapping draws less than daily tapping on the reserve food in the intermediate quarters, and therefore to some extent excuses the immediate tapping of the third, but the system would be improved by the adoption of a resting period after the first two years. The yields of rubber from the successive quarters have not been recorded. The experimental results hitherto recorded unfortunately do not relate to any practicable tapping system. A resting period after the first two years' tapping would restore the food in the third quarter, but unless unduly prolonged it would not restore the latex to its original condition.

Several estates obtain good results by the following variation of the system just described. The first quarter is tapped every other day for three months ; tapping is then transferred to the opposite quarter for the next three months, after which the first side is again tapped for three months, the change being repeated every three months until both quarters have been completely tapped. By this system the two opposite quarters last two years, but the three months' interval allows a considerable thickness of cortex to be renewed over the three inches which have been excised, and therefore some food can travel down the stem before the wound is renewed. This method is objected to on the ground that when the tapping is transferred from one side of the tree to the other, several cuts must be made before a maximum flow of latex is obtained. But, judging from the available records, the increased quantity of rubber in the latex should compensate for that. On the whole, this method appears preferable to the former. But an interval of rest should be allowed after the second year.

If the full or half herring-bone extends over half the circumference, the tree should be rested after the first side has been tapped until an appreciable thickness of renewed cortex has been formed. If the other side must be tapped immediately, two strips of untapped cortex should be left along either side of the tapped area, the tapping on the other side being restricted to about one-third of the circumference. But tapping half the tree is only excusable for financial reasons—it cannot be defended on botanical grounds—and when the immediate advantage has been secured there should be no reluctance to rest the trees. It must be remembered that when one side of the tree is tapped, latex is drawn from the other side ; it is therefore more profitable to tap another group of trees than to tap the other side of the same trees.

The Inverted V.

This is a modification of the half herring-bone, applied to half the tree. If the oblique cuts of that system are long a large amount of 'scrap' is produced. Therefore, instead of one long cut, two oblique cuts in the form of an inverted V are made so that the latex flows down both ways. Of course, two vertical channels are required, but the same two are afterwards used when tapping the other side of the tree. It has also been claimed that this system reaps the advantage in yield of two short cuts over one long cut of equivalent length ; but this claim is obviously a misinterpretation, since the two short cuts are continuous. From a practical standpoint, the necessity of having two collecting cups condemns this system, while from the botanical side it is slightly worse than either the full or half herring-bones, even if we suppose that the vertical channels have no effect on the lateral flow of food. For the latex and food must pass obliquely *upwards* if they are to pass into the bridges of cortex between the cuts.

This system is often worked as a four-year system, one side being tapped continuously for two years, and the other side for the next two years. The disadvantage of immediately tapping the second side has been already pointed out ; until the cortex on the first side has attained a fair thickness, the tree is practically girdled ; and the yield of latex from the second side is much less than that from the first. The continuous two years tapping on one side is also a disadvantage, since the roots on that side do not obtain any food from the crown during that time ; and when tapping is transferred to the other side they are called upon to supply water, &c., to the whole tree at the time when they are at their worst. As a choice of evils, the system of changing from one side to the other every six months is preferable to tapping for two years on one side, because the roots will obtain some food during the six months' rest, *i.e.*, after the cortex is partly renewed. But monthly changes are far too quick, and practically girdle the whole tree all the time.

THE NORTHWAY SYSTEM.

This system was brought out in Ceylon in 1909, and was given an extensive trial on a large number of estates. Only the lowest eighteen inches of the stem was tapped, and all the latex was obtained by the pricker. The lower part of the trunk was scraped smooth, and a channel was built round the stem near the base by inserting small pieces of sheet iron, about two inches long and half an inch broad, overlapping one another. At a height of eighteen inches, the pricker was run horizontally round the stem, leaving one ring of incisions; and at the same time another similar ring was made at a height of nine inches. The latex from these two rings ran down into the channel at the base, and, in order to facilitate this, the lower part of the stem was sprayed with water by means of an ordinary hand syringe fitted with a mist nozzle. On the following day two more rings of incisions were made about one-sixth of an inch below the previous two; and this was repeated daily until all the cortex of the lowest eighteen inches had been operated upon. It was claimed that the estimated yield by other methods could be obtained by this system in sixty days, at less cost and with less damage to the tree. There was also a widespread belief that this system could be safely applied to trees which were too young to be tapped by any other method.

The effects of the pricker have been already referred to. In the Northway system, the pricker first employed had blunt points, the sides of the teeth being sharp, but the ends truncated and lozenge-shaped in section. This pricker had to be forced into the cortex, and each tooth made two small pricks, with a bruise between, on the wood; it required so much force to drive it that each tooth penetrated with a distinct jar, and frequently they broke off. The marks left on the wood were usually larger than those caused by the sharp pricker, and the coalescence of the black patches which resulted from the death of the cambium round each incision sometimes caused the death of the tapped cortex; consequently the blunt pricker was soon abandoned, and the old sharp pricker readopted.

Comparatively few trees were killed by the insertion of the metal channelling; but to obviate further danger of this, a special tool for inserting it was invented. It was also found necessary to provide a tool which marked half-a-dozen horizontal and parallel lines round the stem, because the coolie could not be trusted to run the pricker horizontally, and therefore minced up the bark by cutting in all directions.

Immediately after this system had been introduced, attention was directed to the character of the renewed cortex after the use of the pricker, and to the fact that the tree was girdled by being tapped all round at the same time. Fitting's account of the structure of pricked cortex was also translated and published for the benefit of Ceylon planters. It has been stated that this action caused the abandonment of the system, but, though it would be indeed gratifying to believe that, there is not the slightest doubt that it was abandoned because the yield of rubber was not up to expectation, and the cost of applying it on the average estate was prohibitive.

Tapping Young Trees.

The adoption of the quarter system necessitates waiting until the trees are six or eight years old before a quarter section is sufficiently large to be worth tapping. Hence it is not surprising that methods are sought by which the trees can be tapped when their girth is less, and some return obtained at an earlier stage. The recent high prices of rubber have intensified this desire, and consequently trees which measured only fourteen inches in girth at a height of one foot have been brought into tapping. Of course, the only possible advice which a botanist can give in this matter is 'don't'; but it is quite certain that this advice is Utopian at present. The matter is one which will right itself when large numbers of trees have come into bearing, but to meet the current demand the following methods may be quoted and examined.

The Basal V.

By this system a single V is cut, extending over one-half the circumference of the stem at a height of one foot or eighteen inches, and tapping is continued by reopening this cut every day or every other day until the base of the tree is reached. This interrupts the current of food down to the roots over rather more than half the circumference, and draws upon the latex in the other side of the tree. When the first side has been completely tapped, the other side is treated similarly. Thus for some time the tree is girdled, and the yield from the second side is less than that from the first. For both these reasons, therefore, the second side should not be tapped immediately after the first. It would be preferable to rest the tree, after the first side had been tapped, until it was large enough to be tapped on the quarter system.

Basal Oblique Cuts.

This method requires two oblique cuts, each extending over one-quarter, or slightly more than one-quarter, of the circumference, parallel to each other and on opposite sides of the tree. It is claimed for it that it leaves the maximum space on the opposite sides of the tree between the incisions, that there is no distortion of the tree, and that it can be followed by the single herring-bone. Obviously the direction of the incisions, whether parallel or in opposite directions, makes no difference. The method is certainly better, as far as the welfare of the tree is concerned, than the basal V, but it does not appear to have been tried on small trees. Figures have been published giving the yield of latex when basal incisions are followed by the half herring-bone; but as the trees averaged twenty-nine inches in girth at three feet, there was no reason why they should not have been tapped on the quarter system from the beginning.

Vertical Incisions.

This is an application of the method employed in tapping Ceara in Java and East Africa. A vertical incision is made, extending from a height of six feet down to the base, and in some cases the pricker is run down the incision. In subsequent tappings similar incisions are made an inch or so apart. The yield of latex obtained from one cut is greater than that from a half herring-bone, but obviously it is impossible to continue tapping so long as by the latter method. No detailed results of this method have been published, nor is there any evidence as to the character of the renewed bark.

Suggested Systems.

Fitting advises that a tree should not be tapped higher than where it attains a circumference of eighteen inches. Gallagher has suggested that young trees which measure eighteen to twenty inches in girth at three feet should be tapped as follows: 'Put on a basal V eighteen inches high and tap every day. This will last a year. The second year put a similar V on the other side. The third year begin on the one quarter in one year system on either of the first two quarters tapped, and put on cuts as high as the girth allows, taking the opposite quarter the fourth year. I depart here in the first two years from the one-quarter system, because (*a*) we know that in trees of five or six years old which have had only one cut put on them the renewed bark is thick enough in two years to be tapped again; (*b*) the cuts are short, and the distance which building material must move transversely is not so great as in later years; and (*c*) the cut on one quarter is too short and the bark higher up is too thin, if two are put on, to tap without considerable wounding.' The chief objection to this method is the absence of any resting period between the tappings on the two sides and again before the adoption of the quarter system. It is quite certain that after the first two years' tapping the tree is not in a condition to endure immediate tapping for a further four years. It would be interesting to know the yield from such a six years' course, and it may be doubted whether it would exceed that from a four-quarter system begun two years later. With regard to the reasons given, it may be noted (*a*) that the possibility of retapping must be judged by the amount of food in the tree and not merely by the thickness of the renewed cortex; (*b*) that most of the reserve food consumed in renewing the cortex is obtained radially from the underlying wood, but if half the tree is tapped the area of cortex affected is relatively *greater* than in later years when only one quarter is tapped. It would seem preferable to tap by the quarter system up to where the tree measures eighteen inches in girth, with three cuts in the first year, and to add further cuts higher up on the succeeding quarters when practicable.

Mr. J. Sheridan Patterson has proposed a method by which trees may be tapped when sixteen inches in girth at three feet from the ground. He states that, if cultivated, clean-weeded, and manured, a good four or five-year-old rubber tree will put on from five to six inches in girth in twelve months, and he suggests, therefore, that this increase should be allowed for in planning a tapping system. According to his proposed method, a tree, sixteen inches in girth at three feet, should be tapped on half the circumference, by the half or full herring-bone, by two cuts only, the bottom cut fifteen inches from the ground, and the second cut twelve inches above that. These would last for twelve months, and then, instead of tapping the other side of the tree, 'as is now almost universally done,' three more cuts, a foot apart, should be put above the first two, and tapped for another twelve months. Tapping would thus be confined to one side of the tree for two years. The tree should now be twenty-eight to thirty inches in girth at three feet. Two-thirds of the untapped side of the tree should now be taken, *i.e.*, a vertical strip nine to ten inches wide, and tapped for two years on the same plan as the first side. By the time that strip was finished the remaining strip would be wide enough to tap, and it should be tapped on the same plan as the previous strips for two years. By this method the renewed cortex on the first strip would be five and six years old when the first cycle had been completed, and there would always be a wide strip of cortex more than two years old connecting the crown with the base of the tree.

Several possible objections to the above system may be pointed out. In the first place, the estimated increase in girth appears to be too large; Willis, in 1907, stated that no estate had reached an annual average increase of four inches, and the best was three and a half. Further, the method of tapping for two years on the same strip is to be deprecated, since it cuts off the roots on that side from the crown for the whole of that time; the untapped cortex on the other side does not convey food to the roots on the tapped side, even though three inches of untapped cortex are left round the base (*vide* Fitting's experiment, in which no starch was found in the cortex within six inches below the lowest cut, though only a quarter of the tree was tapped). Again, it must be determined by experiment whether the yield in the second year's tapping on each strip is sufficient to warrant such tapping. Ten old trees at Henaratgoda were tapped over half their circumference by three cuts on the lowest three feet, and yielded 12,600 grammes of rubber in 120 tappings, extending over a period of about 140 days ; the other side was then similarly tapped for 180 tappings in about 220 days. Tapping was then transferred to the first side, where three more cuts were made, one foot apart, above the old cuts ; and the yield in 120 tappings was 8400 grammes. The yield from the second tapping on the first side was therefore only two-thirds that from the first tapping, though that side had been 'rested' about eight months. If the upper tapping follows immediately after the lower, a greater decrease may be expected.

CHAPTER VI.

Tapping Experiments and their Teachings.

It is not intended in this chapter to record the yield obtained on that or the other estate, or to give lists of abnormal yields from single trees, but to examine the existing records and determine, as far as possible, what information they afford on the general principles which should govern tapping. The pathological effects of tapping have already been pointed out, and in many quarters attempts are being made to modify tapping systems so as to reduce these effects to a minimum; but, quite apart from that, there are doubtless certain fundamental principles which must be observed if any system is to be worked to the best advantage.

It must be admitted that the material available at present is extremely scanty, and affords little more than indications of lines for further research. Tapping experiments have hitherto been devoted to ascertaining what amount of rubber could be obtained in a given time, and the data which would have enabled general conclusions to be deduced from the results have, as a rule, not been recorded. We have had to be content with the record of the total yield without knowing more than one or two of the conditions under which it was obtained. Moreover, very few of the recorded experiments have observed the ordinary laws of experiment, and therefore the majority are experiments only in name; they do not furnish any reliable results. And, furthermore, in those experiments which have been conducted, more or less, in accordance with exact experimental methods, tapping systems have been employed which are now quite out of date, or are quite unsuitable for estate work. Reasons for rejecting some of these experiments have already been published; it is not intended to discuss all of them here, but only to cite those which may throw some light on general principles.

The Direction of the Cut.

Parkin determined the amount of latex which could be *collected* from vertical, horizontal, and oblique cuts respectively, when cuts of each type were made at the same level on each tree. He found that—

(*a*)	21 vertical incisions gave 8·5 c.c. of latex.	
	21 oblique „ „ 16·5 c.c. „ „	
(*b*)	14 horizontal incisions gave 6·0 c.c. of latex.	
	14 oblique „ „ 12·0 c.c. „ „	

In both experiments the oblique incision yielded about double that of the other.

It is to be noted that these figures merely give the latex collected, not the amount which exudes from the cuts. It is a *practical* result which makes no addition to theory. As Parkin notes, and as theory demands, 'there is a greater output of latex from the horizontal cut, yet much more dries on the wound than in the case of the vertical; consequently, the amount which drops into the receiver comes to about the same in the two cases.' The difference between 'practical' experiments and those which advance our knowledge of theory needs to be insisted upon. If the yield obtainable is the chief aim of the experiment, then it may be right to confine one's results to the amount of first-class rubber collected; but if it is desired to establish some theoretical point which may influence rubber tapping—as, for example, the percentage of rubber in the latex at different seasons—then all the rubber, first quality, second quality, and scrap, must be included.

SINGLE V. DOUBLE CUTS.

Parkin also compared the yields of latex from single oblique cuts and double oblique cuts, *i.e.*, V cuts, with the following results:—

Feb. 3, 1899.	21 single oblique cuts gave 15.5 c.c. latex.
	21 V cuts „ 32 c.c. „
Oct. 18, 1898.	12 oblique cuts gave 58·5 c.c. latex.
	12 V cuts „ 60·9 c.c. „
Oct. 20, 1898.	14 oblique cuts gave 32·3 c.c. latex.
	14 V cuts „ 51·8 c.c. „

The first of these experiments was carried out in the dry weather, and the double cuts then yielded about twice as much as the single. In the other experiments, done in the wet season, the single cuts gave practically the same as the double in one case, and about two-thirds that of the double in the other. Parkin concludes that if the latex is running freely the V cut gives little more latex than an oblique cut equal to one arm of the V, but if the flow is small the V may give twice as much as the other. Hence it would appear that in the former case the single cut drains about the same area as a V. The length of the oblique cut is not stated, but it appears to have been only one and a half inches long; if so, the experiment needs repetition with the longer cuts now in vogue, and the yields should be ascertained for each tapping during a fairly long tapping period. Also the yields from V cuts and from oblique incisions of double the length of one arm of the V should be ascertained, *i.e.*, the yield from a full herring-bone and a half herring-bone of the same breadth.

It may be deduced from Parkin's experiment that, when the latex is flowing freely, two small incisions at some distance apart will yield more latex than one incision equal in length to the two put together.

The Yield at Various Heights.

The yield at various heights on the same stem was also tested by Parkin, two single oblique cuts being made at each elevation on each tree.

7 trees.	14 cuts at	½ ft. up	gave	16 c.c. latex.
	14 „ „	3 ft. „	„	11 c.c. „
	14 „ „	6 ft. „	„	13 c.c. „
6 trees.	12 cuts at	½ ft. „	„	8·5 c.c. „
	12 „ „	3 ft. „	„	7·0 c.c. „
	12 „ „	6 ft. „	„	5·5 c.c. „
7 trees.	14 cuts at	the base	„	22 c.c. „
	14 „ „	4 ft. up	„	14 c.c. „
	14 „ „	9 ft. „	„	11·5 c.c. „
8 trees.	16 cuts at	the base	„	30 c.c. „
	16 „ „	2 ft. up	„	19 c.c. „
	16 „ „	6 ft. „	„	16 c.c. „
7 trees.	14 cuts at	1½ ft. „	„	17·5 c.c. „
	14 „ „	3 ft. „	„	16·5 c.c. „
	14 „ „	6 ft. „	„	10·5 c.c. „

These experiments were all done in March, in the dry weather, when the flow of latex was small. This is rather unfortunate, since except in the third and fourth experiments, the figures are scarcely large enough to carry conviction. Fourteen cuts require fourteen collecting vessels, and a difference of 1·5 cubic centimetres in the total yields is only about one-tenth of a cubic centimetre for each collecting vessel. As the results stand, they indicate a gradually decreasing yield of latex as the cuts are placed higher up the tree. 'It may be concluded that there is a greater exudation of latex from wounds made at the base of the trunks of *Hevea* trees than at any higher region; that the exudations from one to five or six feet up the trunk differ little; and that above five or six feet the latex exuded falls off very considerably' (Parkin). Although it is matter of general belief that the latex at the base of the tree is richer in rubber than elsewhere, and that it flows more freely from that region, it is curious to note that, in prolonged experiments, the greatest yield has been obtained from higher parts of the stem.

The Number of Cuts.

Though the half herring-bone is becoming a favourite method of tapping, there have been no experiments which throw any light upon the questions of the best number of cuts to employ, or the influence of one cut on the yield of another. The distance between the cuts is at present arranged merely to suit the arbitrary duration of the tapping period; as a rule the tapping period extends over one year, and one foot of cortex is removed during that time; therefore five or six cuts are placed one foot apart. But this is beginning at the wrong end. We need to know whether six cuts at one foot apart yield more than four cuts at eighteen inches apart, or three cuts at two feet apart, both for the same time and for the same area of cortex. And it would be worth while to compare the yield from the lowest six feet of trunk tapped by six cuts with that from four cuts on the lowest four feet. Further, the actual yield of each cut should be determined throughout a prolonged tapping period: some of them appear to contribute very little, at times, to the yield of a half herring-bone.

In this connection it may be recorded that the inventor of one tapping system states that he obtained no more rubber from three rows of incisions than from two, and that the yield of two rows was not double the yield of one row.

Right *versus* Left-hand Tapping.

For several years it has been contended that more rubber is obtained by tapping to the left than by tapping to the right, that is, if the oblique cuts are made to the left of the vertical channel of the half herring-bone instead of to the right of it. In other words, a cut which slopes up to the left is more productive than one which slopes up to the right. Experiments made on Culloden Estate gave a yield in the former case forty per cent. greater than that in the latter. In many quarters this result is accepted, and all the tapping is to the left, but many deny that there is any difference between the two.

It has been suggested that this effect, which has undoubtedly been observed, is to be attributed not to the tree, but to the coolie—that the coolie finds it easier to tap in one direction than in the other, and so cuts deeper into the cortex and obtains more rubber when cutting in the direction he finds the more difficult. But the following observations afford another explanation. On stripping off the cortex from a dead *Hevea*, the wood will be found to be faintly ridged, more or less vertically, with lines which indicate the direction of the fibres in the stem. Twenty-five *Hevea* stems of various sizes which happened to be lying in my laboratory were examined; and it was found that the fibres sloped slightly up to the right in eighteen of them, while in the other seven they were practically vertical. The latter stems were nearly all nine inches or less in girth, while the former included stems from nine to twenty-seven inches in girth. It would appear, therefore, that the slope occurs in old trees, but not in very young trees. The medullary rays are inclined in the same direction as the wood fibres, and as the general direction of the latex vessels is governed by that of the medullary rays (since they curve round the latter), it would seem to follow that the latex vessels must also slope up to the right. In that case it would naturally happen that tapping to the left would yield more rubber than tapping to the right, since the tapping cut in the former direction would sever more latex vessels. For example, if the latex vessels are inclined to the right at an angle of five degrees with the vertical, and the cuts are made at an angle of forty-five degrees to the vertical, then the cut which slopes up to the left will sever nearly twenty per cent. more latex vessels than that which slopes up to the right.

If the slope does not occur until the trees are old, the conflict of opinion on this subject might possibly be explained by supposing that different experimenters had tapped trees of different ages. It may of course happen that in some trees the fibres are always practically vertical, and it is also conceivable that they may slope up to the right in some trees and up to the left in others, as in the coconut palm. As far as our present knowledge enables us to judge, tapping to the left should yield more rubber than tapping to the right. But if further examination should prove that right- and left-handed trees occur at random, no difference in yield could be expected.

The Percentage of Rubber in Latex.

This has already been referred to in a previous chapter. Bamber and Lock tapped trees, upwards of twenty years old, by a full herring-bone of three V's extending over half the circumference of the tree and to a height of three to four feet. Seven groups were tapped, at intervals of from one to seven days. The percentage of rubber in the latex was about 50 at the beginning of the experiment. In daily tapping the percentage fell fairly steadily to about 26 at the twenty-third tapping, after which it oscillated between 25 and 35 for the remainder of the tappings on that side ; it is probable that these later variations are in part due to the weather. In two-day tapping it fell to about 30 at the twelfth tapping, and subsequently varied between 30 and 36, though there was an abnormal fall to 25 towards the close of tapping on that side of the tree. In three-day tapping it fell to about 32 at the seventh tapping, and subsequently varied between 30 and 38. In the groups tapped at intervals of four, five, six, and seven days respectively, the percentage of rubber fell immediately to about 40, and subsequently varied from about 35 to 45.

The percentage of rubber in the latex decreases as the tree is tapped, but after a certain number of tappings it attains a fairly constant average, rising and falling within narrow limits, apparently under the influence of external conditions. This average percentage is less the more frequent the tapping, but there is not much difference when the interval is longer than three days.

It is to be noted that the system employed in these experiments is more drastic than those now recommended ; and it is questionable whether the diminution of the percentage of rubber in the latex would be so great if the trees had been tapped by the quarter system.

The above results are in direct contradiction to those of a Singapore experiment, in which certain trees were tapped twenty-one times in twenty-four days. The quantity of latex obtained increased from 114 ounces at the first tapping to 338 at the tenth, while the percentage of rubber in the latex showed practically *a steady increase* during the same period, the percentage at the tenth tapping being more than one-third greater than that at the first. As, however, the percentages of rubber were only 27·5 and 20 respectively, it is probable that the water added to the collecting cups was reckoned as latex ; and as the amount of this water would be practically constant, it would reduce the percentage of rubber in the earlier tappings, when the latex yield was small, more than in the later tappings when the latex was more abundant.

Burgess has recorded an experiment designed to show the difference in the quality of the latex from different parts of the same tree. The tree in question was thirteen years old, and was being tapped for the first time, but the number of tappings is not stated. A large root was uncovered, and tapped by a simple three-inch cut : the percentage of rubber in the latex from that region was 43·8. The latex obtained by tapping the basal two feet of the main stem by a full herring-bone contained 44·4 per cent. of rubber, while that obtained by tapping in the same way on the main stem twenty feet above ground contained 39·8 per cent. The differences between these percentages are not large enough to warrant any deduction in the absence of further data. The percentage of resin in the crude rubber from the three regions mentioned showed a regular decrease from below upwards, *i.e.*, it was greatest in the rubber from the roots and least in that from the higher parts of the stem.

Tapping One Side reduces the latex in the Other Side.

In the experiments by Bamber and Lock already referred to, the trees were tapped by three V's, one foot apart, extending over one half the circumference of the tree, on the lowest three to four feet of the stem. When the first side had been completely tapped, the other side of the tree was immediately tapped in the same way ; and after the cortex on the basal three to four feet of the second side had all been excised, tapping was again transferred to the first side, where three similar V's were cut, above the previous three. There were therefore four consecutive tapping periods, on alternate sides of the stem.

In daily tapping, the first side was tapped 120 times in about 140 days, and the yield of rubber per tree fell from 197 grams in the first ten tappings to 70 grams in the last ten tappings. The second side was then tapped 180 times in about 220 days. The yield per tree from the first ten tappings on the second side was only 118 grams, and it fell to 26 grams in the last ten tappings. As the second side was more completely tapped (180 times instead of 120), it is probably unfair to compare the last ten tappings of that side with those of the first; the corresponding tappings on the second side are the 110th–120th, and these yielded 52 grams of rubber per tree. Thus the yield of the first side fell to less than half (197 to 70); and when tapping was transferred to the other side, the yield at the beginning was less than two-thirds of the initial yield of the first side, and the final yield was only about one-eighth. Further, the latex averaged 40 per cent. of rubber during the first ten tappings of the first period, but only 30 per cent. during the first ten tappings of the second period.

In two-day tapping, the first period lasted about 330 days, and the trees were tapped 140 times. The yield of rubber per tree was 154 grams in the first ten tappings, and 42 in the last ten. During the second period, when tapping was transferred to the other side, the yield per tree in the first ten tappings was 76 grams, about half that of the first side. The percentage of rubber in the latex averaged 40·7 in the first ten tappings of the first side, but 30 in the corresponding tappings of the second side.

In three-day tapping, the trees were tapped 130 times in about 470 days in the first period; the yield per tree was 175 grams in the first ten tappings and 60 grams in the last ten. In the second period, the first ten tappings yielded 80 grams, Further details are not yet available.

These results are summarised in the following table:—

When tapped.	First Period.			Second Period.		
	No. of tappings	First ten.	Last ten.	No. of tappings.	First ten.	Last. ten.
Daily... ...	120	197 gms.	70 gms.	180	118 gms.	26 gms.
Every 2 days	140	154 „	42 „	unfinished.	76 „	—
Every 3 days	130	175 „	60 „	„	80 „	—

In all the cases recorded, the yield of rubber is less, and the percentage of rubber in the latex is less, when the second side is tapped than it was when the first side was tapped. This cannot be attributed to external conditions, because the tapping intervals are different, and therefore the change from one side to the other occurs at different seasons in the different experiments. Therefore, since the effect is evident in every case, it can only be attributed to the effect of tapping on the tree. Hence it appears that when one side of the tree is tapped, latex is drawn from the other side, and the untapped cortex is therefore rendered poorer in rubber. It should be determined how far this result is applicable to the quarter system, *i.e.*, whether the tapping on one quarter affects the opposite quarter; it must of course affect the adjacent quarters.

The Relative Value of Different Methods of Tapping.

Though at least one of the methods employed in the following experiments is now obsolete, the results are quoted here because they serve to illustrate other points than those which they were designed to show.

Three groups of trees, about twenty years old, were tapped at Henaratgoda during 1905-6. Each group contained twenty-five trees, and all were tapped from the base to 5 feet 6 inches. A was tapped by the full spiral, B by the half-spiral, and C by the full herring-bone, in each case twice per week. The details which have been published enable us to divide the year's tapping into three periods, and by this means information other than the total yields can be obtained. The yields for the three periods are given in the following table:—

Tapping Periods.	A.		B.		C.	
	No. of tappings.	Yield in lbs.	No. of tappings.	Yield in lbs.	No. of tappings.	Yield in lbs.
Sept. to Feb.	37	50⅞	41	35⅛	39	47 5/16
Feb. to April	20	20 13/32	19	11¾	18	24¾
April to Sept.	34	11 17/32	33	15⅝	35	2 15/16
Sept. to Sept.	91	82 13/16	93	62½	92	75

It will be seen from the numbers of tapping in each period that the trees were not tapped regularly twice per week. B gained two weeks on A in the first tapping period, and A gained one week on C in the second, &c. It follows that the three groups of trees were not all tapped on the same day, and therefore the yields are influenced by different climatic conditions. But in the absence of any other, more accurate, experiment, we must be content with the fact that the tappings were done almost in the same seasons.

At the end of the first period the full spiral has given the highest yield, although the number of tappings is least in that group. At the end of the second period the full herring-bone has given the best result, though only slightly better than the full spiral; the half spiral, which has received the greatest number of tappings, is much inferior to either. But in the third period the full herring-bone falls off in an extraordinary manner, and thus the full spiral comes to have the greatest final yield.

These results are tabulated below in a different form, the table giving the yield per tree per tapping in ounces during each of the three periods.

	A.	B.	C.
September to February ...	0·88	0·55	0·78
February to April	0·65	0·40	0·88
April to September... ...	0·22	0·30	0·05

For the first 37 tappings, A yields 0·88 ounces per tree per tapping: for the next 20 tappings it yields 0·65 ounces, and for the last 34 tappings it yields 0·22 ounces. B follows a similar course. But C, after yielding 0·78 ounces per tree per tapping for the first 39 tappings, *rises* to 0·88 ounces for the next 18 tappings, and falls to 0·05 ounce for the last 35 tappings. This anomalous result throws some doubt on the validity of the figures. The rise in the second period is not shared by either of the other two groups, and it occurs in the dry weather when it would least be expected. Nor can the low yield of the last thirty-five tappings be attributed to exhaustion; for the total number of tappings is only ninety-two, the trees were tapped only twice per week, and only three to four inches of cortex had been removed (Wright, *Tropical Agriculturist*, December 1906). Nor is it to be attributed to the individual peculiarity of the trees, for it is not the yield of one tree, but the average of twenty-five.

If the figures are correct, we must conclude that the full herring-bone is the most exhaustive of the three methods of tapping, for after fifty-seven tappings the trees yield hardly anything.

It has been deduced from this experiment that the full spiral is the best method to adopt in thinning out estates which are too thickly planted, since it removes the maximum quantity of cortex in a given time. But it does not follow, even if the results quoted above are accepted, that it yields the greatest amount of rubber. In these experiments the full spiral yielded 82 lbs. of rubber, while the full herring-bone yielded 75 lbs. But the full spiral tapped all round the tree, while the herring-bone dealt only with one side. It would be possible, therefore, if it were desired to injure the tree, to put another herring-bone, perhaps smaller, on the other side; and a small yield from the second herring-bone would suffice to make that method the better. Or tapping might be transferred to the other side when the yield from the first herring-bone had diminished to an unremunerative quantity. It should, however, be pointed out that ordinary estate practice does not confirm this remarkable falling off in the yield of the full herring-bone.

The Yield of Rubber from Different Parts of the Stem.

An experiment on this subject was also conducted at Henaratgoda during 1905–6. The trees were all tapped by the full herring-bone with oblique cuts one foot apart. The results were as follows:—

Group.	Number of trees.	Tapped from.	Number of tappings.	Yield per tree in lbs.
C	25	base to 5½ ft.	92	3
M	2	6 to 16 ft.	95	8·7
N	2	10 to 20 ft.	94	12·2
O	2	20 to 30 ft.	94	8·7
L	1	base to 30 ft.	93	14·5
W	2	base to 50 ft.	84	15

Tapping the basal 5 ft. 6 in. gives 3 lbs. of rubber per tree; with rather less than twice the length of stem, 6–16 ft., the yield is nearly three times as great, and the same is true for the length, 20–30 ft.; from 10–20 ft., again rather less than twice the length tapped in group C, the yield is more than four times as great. Only in the last two groups is there any falling off. When the length tapped is increased about five and a half times the yield is less than fivefold, and when it is increased nine times the yield is only fivefold. But in the last case the tappings are fewer. These experiments certainly do not support the claim that the greatest amount of rubber is obtained by tapping the basal 6 ft. Of course, several data are missing; in particular there is no information as to the girths of the trees, but as they were specially selected for this experiment it is to be expected that they were fairly uniform.

The experiment is to some extent vitiated by irregular tapping. The tappings can be divided into three consecutive periods, as in the preceding experiment, and in that way we obtain the following information:—

NUMBER OF TAPPINGS.

Group.	Sept. to Feb.	Feb. to April.	April to Sept.
C	39	18	35
M	16	29	50
N	16	28	50
O	16	28	50
L	23	24	46
W	8	29	47

During the first twenty weeks C was tapped regularly twice per week, but three of the other groups were tapped less than once per week, and W was only tapped once in two and a half weeks. In the second period C is again tapped regularly, but the other groups are tapped three or more times per week, and the third period suffers in the same way. Apparently all the groups except C were tapped more rapidly in order to bring the total number of tappings up to the standard, and as this was done especially in the second period, which includes the dry season, their yields suffer in comparison with C. Further, the more frequent tapping, since the total numbers of tappings are approximately equal, reduces the yield of the other groups as compared with C. Everything here is in favour of Group C, but the figures do not prove that it is the best.

As the experiment stands, it appears to prove that the greatest yield of rubber is obtained when the centre of the tapped area is about one-quarter the height of the tree from the base, since these trees are sixty to seventy feet high. This is rather a curious result; for if we regard the main stem as a cone, then its centre of mass is situated at one-quarter the height of the tree from the base; hence the greatest quantity of rubber is obtained by tapping equally above and below the centre of mass of the stem. Or again, if we regard the stem as a hollow cone filled with liquid the resultant pressure of the liquid on the containing surface is situated along a line one-quarter the height of the stem from the base. But it is probable that these coincidences are merely fortuitous.

The Yield obtainable by Tapping at Different Intervals.

The first available experiment on this subject is another Henaratgoda experiment of 1905–6. Five groups of trees were tapped by the full spiral up to 5 ft., or 5 ft. 6 in.; with the following results :—

Group.	No. of trees.	Tapped.	No. of tappings.	Yield per tree in lb.	Oz. per tree per tapping.
D	5	6 times per week	270	11·0	0·65
E	5	3 " "	136	12·5	1·47
A	25	twice "	91	3·3	0·58
F	5	once "	44	3·8	1·38
G	5	once per month	11	0·625	0·91

These figures apparently prove most conclusively that alternate-day tapping yields more in a given time than daily tapping. As a rule it is considered that alternate-day tapping halves the labour, and gives more rubber *per tapping* than daily tapping, but that it does not give twice as much, and therefore the total yield at the end of the year is less, the gain being in the labour per pound of rubber obtained. But, in the present case, not only is the labour halved, but the yield per tapping is more than doubled.

It is, however, doubtful whether sufficient information to admit of valid conclusions has been published. The trees were said to be 15–20 years old, but their girths were not recorded. From the figures in the last column it would seem impossible that the trees were even approximately similar in that respect. It would be expected that yield per tree per tapping would show a gradual increase as the tapping interval was lengthened, but weekly tapping is no better than two-day tapping, and three-day tapping is the worst of the whole series. The monthly tapping may be left out of account, as there could be very little 'wound response' under such circumstances.

Further light is thrown on these results by dividing the tapping period into three, as in the previous Henaratgoda experiments quoted. The yield per tree per tapping, in ounces, during each period is given in the following table :—

Group.	Sept. to Feb.		Feb. to April.		April to Sept.	
	No. of tappings.	Yield.	No. of tappings.	Yield.	No. of tappings.	Yield.
D	112	0·88	56	0·67	102	0·39
E	56	1·49	27	2·78	53	0·79
A	37	0·88	20	0·65	34	0·22
F	18	1·52	10	1·29	16	1·29

The yield per tapping in three-day tapping is equal to that in daily tapping during the first period, and practically equal in the second, but it falls off much more in the third, though its number of tappings was less. But if A is omitted the yields of the other three groups agree with expectation in the first and third periods, that is, the yield per tapping in weekly tapping is greater than in alternate day-tapping, and the latter is greater than in daily tapping. But in the second period this agreement does not exist.

In Group D there is a regular decrease as tapping proceeds. Group A shows a similar decrease. F shows a drop in the second period, but the yield per tapping in the third period does not decrease further, probably because the total number of tappings is small, and also because the third should, for climatic reasons, have been a better tapping season than the second. But E does not fall in with these series. In the first period its yield per tapping is less than twice that of D, but in the second period it rises to more than four times that of D, to fall again to about twice in the third period. This rise cannot be due to external conditions, since it does not occur in the other groups, and it is the more inexplicable since it occurs in the worst season. In the absence of any explanation, it would be unsafe to accept these results without further confirmation.

Bamber and Lock's experiments at Henaratgoda during 1908–9 afford further information on this subject. After tapping for about a year and a half the following yields were obtained, in grams per tree :—

Tapping interval in days	1	2	3	4	5	6	7
Yield of rubber in first ten tappings	197	153	175	173	154	200	156
Total number of tappings ...	435	230	150	110	90	70	60
Total yield ...	3136	1959	1634	1295	1231	1094	769
Yield per tree per tapping	7·2	8·5	10·9	11·8	13·5	15·6	12·8

The yields of the first ten tappings have been included to show that the groups of trees were not of equal value. Group 2 is evidently much inferior to Group 1, for its yield in the first ten tappings is little more than three-quarters that of Group 1, in spite of the longer interval. Similarly Groups 5 and 7 are poor. It must also be pointed out that tapping was transferred to the other side of the stem, *i.e.*, to an area richer in rubber, after three feet had been excised. Group 1 has had the advantage of two such transfers, and Groups 2 and 3 of one each, while the original side is still being tapped in the remaining groups. These transfers tend to increase the average yield per tree per tapping. In spite of this, however, the yield per tree per tapping steadily increases as the interval is increased until the weekly tapping is reached.

The foregoing experiments show that the yields *per tapping* in alternate-day tapping and three-day tapping are greater than that in daily tapping, when the different series of tappings are carried on for the same time. According to the 1905–6 experiment, the *total* yield in alternate-day tapping is also the greater, but according to the 1908-9 experiment it is much less. The latter is the more probable.

But there is another aspect of the question. We require to know what the yield in the two cases will be when both series are continued *for the same number of tappings*. It is not correct to double the yield obtained in the alternate-day tapping of the experiments just quoted, for it is quite certain that the trees would not continue to yield at the same rate if the number of tappings were doubled. To solve this question the trees must be actually tapped; and here the difficulty arises that the second half of the alternate-day tappings may be subject to weather conditions quite different from those which governed the first half and the daily tappings. However, if the tapping periods are long—say, one year and two years—such differences would, to some extent, be neutralised.

The tapping experiments now in progress at Henaratgoda serve as an illustration. In the group which is being tapped daily the whole of the cortex of the lower six feet of the stem will have been operated upon in about two years; and if the view that the renewed cortex must not be tapped until it is four years old is accepted, these trees must be 'rested' for the next two years. But the group which is being tapped on alternate days will not be completely tapped until the end of four years. The former system demands daily tapping and two years' rest, while the latter requires alternate-day tapping and no rest. The question to be decided is, which gives the higher yield for one complete cycle of tapping. Of course, there is no doubt that both systems are bad in the present case, since the tapping cut extends over half the circumference of the tree; but the question is one which arises in other, more conservative, systems.

The result of this Henaratgoda experiment, as far as the first two groups are concerned, will not be available until the end of four years from the beginning of the experiment, and if they are to be continued to the full cycle of the weekly tapping, *i.e.*, Group 7, they will require fourteen years for completion. From the results already published the following may be quoted, the figures indicating the yield per tree in grams.

Tapping interval.	Yield in 110 tappings.	Yield in 120 tappings.	Yield in 230 tappings.
1 day	1189 gms.	1260 gms.	1941 gms.
2 days	1134 „	1186 „	1959 „
3 days	1333 „	1403 „	—
4 days	1295 „	—	—

Up to the present, therefore, the results would appear to indicate that the yield of rubber in the same number of tappings is greater in three- or two-day tappings than in daily tapping. In three-day tapping the yield is 12 per cent. greater at the end of 110 tappings, and 11 per cent. greater at the end of 120 tappings. The yields in alternate-day tapping are at first less than in daily tapping, but it must be remembered that this group is inferior to the rest; after 230 tappings the yields are practically equal, though Group 2 was nearly 25 per cent. worse than Group 1 at the beginning (see p. 122).

On the system of four quarters in four years, one would, from a pathological standpoint, recommend tapping every day and resting the tree half the time, as advised by Fitting, or tapping every other day continously and resting the tree after two quarters had been tapped. The latter should yield the larger quantity of rubber, the labour being the same in both cases.

At a recent conference in the Federated Malay States, various opinions were expressed on this subject. One speaker stated that in a series of experiments extending over six months, alternate-day tapping gave slightly the best result in the first three, but, during the second three, tapping every day gave the bigger yield. Another stated that after daily tapping for six months he had to return to tapping every other day, while the experience of a third was directly the opposite of that. According to another speaker, daily tapping yielded very little more latex than tapping on alternate days. Of course, no valid conclusion can be arrived at unless both sets of trees are tapped at the same time, and it can surely rarely happen that the yield from alternate-day tapping is greater or even equal to that from daily tapping when both tappings are carried on *for the same time*. To obtain such a result the yield per tapping in alternate tapping must be double that in daily tapping.

On one estate in Ceylon tapping every three days is said to give the best result and has been adopted.

The Diminution in Yield during Tapping.

A glance at the tables on pp. 115, 117, 121 will show that the yield, in any tapping experiment concerning which we have exact information, falls off considerably as tapping proceeds. In full herring-bone tapping over half the circumference (p. 115) the yields per ten tappings fall to 36, 27, and 34 per cent. of the original yields; and when tapping is transferred to the other side the final yield on that side is only 13 per cent. of the initial yield of the tree.

In the experiments recorded on p. 117 the full-spiral after 57 tappings only averages, for the next 34 tappings, one-quarter of the average yield of the first 37 tappings. The half-spiral maintains a more constant yield, and its average yield in the last 33 tappings is more than half the average yield of the first 41 tappings. But the average yield of the full herring-bone for the last 35 tappings is only 6 per cent. of the average for the first 39 tappings.

The experiments quoted on p. 121 show a similar reduction, though the results are extraordinarily contradictory. The fall is not greatest when the trees are most frequently tapped, as would be expected. Attention has previously been directed to the anomalies in these results.

It is, of course, probable that when the quarter system is adopted this falling-off in yield will not be so pronounced. The full-spiral, and probably the half-spiral, in the foregoing experiments, tapped all round the tree at the same time, and therefore were more likely to exhaust the latex rapidly. But the herring-bone dealt with one side of the tree only, and the results of that system will undoubtedly be applicable in many instances at the present day where half the circumference is tapped. And until further evidence is forthcoming with regard to the quarter system, it will be safer to assume that a similar diminution occurs in that case also, though probably not to the same extent.

In the full herring-bone tapping with a three-day interval, quoted on p. 116, the yield of 25 trees in the first 57 tappings was 72 lbs.; in the last 35 tappings it was just under 3 lbs. It has been recorded that only three to four inches of cortex had been excised; if so, another 180 tappings were possible, but even if the rate of yield had been maintained, the 25 trees would only have given about 15 lbs. in the two years which those extra tappings would have occupied; or—to put the same fact in a different way—840 tappings (if that were possible) would be required to produce the same yield as the first 57. This at once raises the question whether it is not better to rest the trees until a better yield can be obtained, than to continue tapping until all the cortex has been excised.

The foregoing is an extreme case, and it will no doubt be said that any planter would have stopped tapping those trees when the yield fell off so considerably. But on the other hand, such information does not reach the planter immediately, especially when young trees are continually being brought into tapping. When the yields per field are regularly tabulated, any serious diminution can be detected, and tapping stopped if necessary, but the real question here is whether it is not better, both from the standpoint of labour and of yield, to arrange the tapping system so that the trees, or at least the tapped area, is periodically 'rested.' At the present time, when labour is sufficient, trees comparatively few, and the price of rubber high, it no doubt pays to obtain every possible ounce of rubber. But when more trees are available, and the problem of economical working comes uppermost, it will most probably be found that it pays better to have two sets of trees, one in tapping and one resting, for each gang of coolies, rather than two sets both in tapping with double the number of coolies.

Resting periods are out of fashion at present. But there is no doubt that they will have to be reintroduced if rubber plantations are to remain in existence for any length of time. Ridley estimates the life of *Hevea brasiliensis* at sixty years, and considers that it will continue to yield fully all the time if the trees are properly spaced; but the tapping experiments at Singapore have as a rule been confined to twenty-five to thirty consecutive tappings, either daily or every other day, and no group has been tapped more than ninety times in one year. The tapping periods in that case are therefore only one-quarter to one-twelfth of the resting periods. In contrast to this, a Ceylon estimate, published in 1907, assumed that if the trees were planted two hundred to the acre, and were tapped to yield 1 lb. of rubber per year after the fifth year, their life would be eight years more! If this latter estimate was made on the assumption that the trees would be tapped continuously by the full spiral it was very probably correct.

Even on the one quarter per year system, the tree cannot be expected to survive many four-year periods, if one follows the other immediately. When the tree is first tapped it contains reserve food which has been accumulating for five or six years; and, as each quarter is operated upon, it draws on the reserve food in the wood immediately behind that strip of cortex, as well as that in the cortex immediately round it. When the cortex has renewed on any strip, the tree again stores food in the wood behind it; but when the four-year period is concluded, reserve food is deficient in the wood behind the strip last tapped, as well as in the cortex on either side of it. Therefore the tree is relatively poorer in reserve food than it was four years previously; and if the next four-year period is begun immediately, it starts at a disadvantage compared with the previous period. Obviously this disadvantage is cumulative, and it would not be safe to calculate on being able to tap for more than two or possibly three such periods, if no resting period intervenes.

Fitting has already advocated resting periods in order to avoid excessive injury to the tree. It is probably too much to expect that they will be adopted before some estates begin to suffer; and it is curious to note that even those who profess allegiance to Fitting's views altogether reject his scheme of tapping. In the system he advises, a quarter is tapped for two months, rested for two months, tapped again for two months, and then rested for six months. Moreover, the whole tree is rested; tapping is not transferred to another quarter while the first is not being tapped. And he also contemplates a resting period at the end of the four years. But, though considerations of health may not carry much weight, the question of yield does, and it is most probable that the reintroduction of resting periods will be brought about by the diminution in yield which follows continuous tapping.

Up to the present there have been no valid experiments which demonstrate the value of resting periods of different duration, or at different stages in the course of tapping.

The Variation in Yield at Different Seasons.

Information on this subject is confined to general opinions founded on the results of estate work, and the various factors which may influence the result are not always considered separately. It is generally believed that the yield is greater in wet weather than in dry, and that the response to rain follows in two or three days. But the actual amount harvested during the wet months may show a decrease, both because the latex may flow over the bark instead of along the cuts, and because tapping is sometimes stopped in wet weather. In the Federated Malay States the rainfall is more regularly distributed than in Ceylon, and therefore this effect may not be so marked.

Mr. Baxendale has recorded that on one estate in the Federated Malay States the yields, both in 1909 and 1910, were highest in February and March when wintering was general. In Ceylon 'wintering' occurs in the dry months, and it is therefore doubtful whether any observed effect is due to the 'wintering' or to the drought. W. N. Tisdall states that the best yields in Ceylon are obtained for the three or four months preceding the wintering stage, and that in the wetter districts about two-thirds of the crop is obtained from July to December. Whether fruiting affects the yield or not is a disputed question.

It has already been pointed out that the effect of the various phases of the tree on the actual quantity of rubber in it must be decided by examinations of the cortex rather than by tapping experiments. Until that is determined, any experiment designed to show the effect of the season on tapping can only show the joint effect of the weather and the phase of the tree. Some experiments on this subject have been recorded, but either the data published is insufficient or the experiment has been varied to such an extent that the result cannot be taken to prove anything. Further, the systems employed are so drastic that their effect masks everything else.

One experiment was carried out at Henaratgoda in 1905-6. Two groups, each of five trees, were selected, and one of them was tapped continuously from October, 1905 to September 1906, while the other was tapped from February 1906 to September 1906. Thus, in the first group, tapping began in the wet weather just after the seed-crop had matured, and was continued through the dry weather (the period of defoliation), and the ensuing wet months, April, June, July; while in the second group, tapping began at the time of leaf-fall. Both groups were tapped by the full-spiral on the lowest five feet of the stem. The yields in ounces per tree per tapping are given in the following table; the tappings were continuous, but they are divisible into three periods :—

When tapped.	Group I.		Group II.	
	Number of tappings.	Yield as above.	Number of tappings.	Yield as above.
October to February...	100	0·89	—	—
February to April ...	57	0·62	68	0·65
April to September ...	100	0·46	103	0·72

In Group I., the yield per tree per tapping falls steadily throughout the year. In Group II., the yield during the dry season is less than that in the ensuing wet season; this is most probably entirely a weather effect, not evident in Group I. because of the effect of the greater number of tappings. If anything, this result tells against the view that the rubber is consumed in the manufacture of new foliage, fruit, &c., since the fruit matures during the third period.

The following experiment is due to Tromp de Haas. Three groups of trees were tapped at different seasons in 1903, and again in 1904; the number of tappings is not stated definitely, but it appears to have been about ten in each case.

	1903.			1904.		
Group—	I.	II.	III.	I.	II.	III.
Tapped	Jan. to Feb.	June.	Sept. to Oct.	Sept. to Oct.	June to July.	Feb.
Rainfall ...	324 mm.	197 mm.	379 mm.	711 mm.	403 mm.	250mm.
Rainy days ...	14	7	9	17	13	9
Surface tapped	12·2 sq.m.	9·26 sq.m.	12·4 sq.m.	12·2 sq.m.	9·26 sq.m.	12·4 sq.m.
Total yield in grams ...	7115	4318	10,482	6718	4678	10,697

The yields of the three groups are in the same order in each year, despite the alteration in the tapping season, and the yields of each group in both years are practically the same. Tromp de Haas notes that both years were unfortunately wet years, and concludes that in the wet years it does not matter when the trees are tapped.

Renewed Bark.

Fitting has laid down the criterion which should govern the determination whether a renewed bark can safely be tapped or not, viz., that the cortex and the wood behind it should have been refilled with reserve food. He advises that at the end of a complete cycle of tappings a tree should be felled, and the distribution of starch in it determined by means of a solution of iodine. 'If the reservoirs in the wood *and* bark have been refilled, there can be no objection to the continuation of tapping, provided that the latex exuding during the fresh tapping period satisfies in quality and quantity all fair demands.' 'These examinations ought to be repeated at least every four years, at the end of each fourth tapping period.' Wood which contains starch is coloured black in mass by iodine; if it does not contain starch it turns yellow-brown under the same treatment. It may be noted that in Fitting's system of tapping there is a rest of six months before this examination is made.

The general opinion is that renewed bark should not be retapped until it is four years old. It will probably be found that this is quite a safe estimate, though there are, as yet, no records of any examination. Renewed bark from a Henaratgoda tree was found to be full of starch three years after tapping. This tree had been tapped by the full herring-bone, extending over half the trnnk; but, on the other hand, it had been tapped only on one side, and it was about twenty years old when tapped for the first time. The examination was made in October, just after the seed crop had ripened.

In some quarters it is held that two years is an ample allowance for the first renewal, and three years for subsequent renewals. But these estimates have not been based on any such investigation as is recommended. In general, reliance is placed merely on the thickness of the renewed bark without any consideration of the reserve food in and behind it.

CHAPTER VII.

THE ART OF EXPERIMENT.

SEEING that so many problems in *Hevea* cultivation must be decided by direct experiment in the field, it has been thought advisable to indicate a few of the points which must be observed by an experimenter if he wishes to obtain results which are likely to approximate to the truth. During the last century—one might almost say during the latter half of it—experiment in science has advanced from the mere record of observations of events which happened at random to carefully controlled investigations in which each step is designed to prove some particular point. Instead of allowing an experiment to run its own course, influenced by numerous factors each of unknown effect, the modern investigator seeks to discover all the factors which may affect the result, and either determines their effects separately or devises some method which excludes or neutralises them. From the accounts of such investigations there has been evolved what may be termed an art of experiment. We profit by the experiences and mistakes of our predecessors, and by studying their work we are enabled to decide how an experiment should be conducted, what factors may influence the result and how they can be eliminated, what our results may be taken to prove, and to what degree they may be regarded as correct. It must be admitted that experiment in agriculture has not advanced to the same degree of excellence as experiment in other sciences, and it might be considered that the uncontrollable factors which influence field experiments have contributed to that; but recent progress would appear to show that the real reason has been a dearth of investigators capable of conducting experiments in such a way that any valid conclusions could be based on their results.

Selection of Material.

All selection of material should be done at the beginning of the experiment. If an experiment is conducted on a large scale, and only part of it finally selected, the result depends upon the personal opinion of the experimenter, which above all things is to be avoided.

When it is desired to compare the yield of two plots under different systems of cultivation, &c., the plots selected for the experiment must be as nearly as possible equal in respect of size, situation, number and age of the trees, and any other local conditions. This is an elementary precaution, but it is often overlooked; in one published experiment which dealt with the effect of various manures on the growth of *Hevea,* all the plots were situated on the same slope, but they were taken parallel to one another across the slope instead of up and down, and consequently no valid comparison could be made. When the plots have been selected, all the trees on each must be taken into account; there should not be any further selection within the plot.

But experiments on *Hevea* will for some time yet deal chiefly with the yield obtained by various tapping systems, or the effects of tapping at different intervals, or the influence of resting periods for different periods; and in such cases the unit must be the tree, not the acre. The trees selected should be as near to one another as possible, so that they are all subject to practically the same soil conditions, and they should, of course, be as equal as possible in age and girth. Equality of age is easily secured, but it is difficult to get even approximate equality in girth if many trees are wanted. Probably the best way of finding suitable trees is to map out the position of every tree on the plot selected for experiment, and to mark the girth of each at the point which denotes it on the plan. Selections can then be made or discarded without much trouble. Attempts have been made to avoid this difficulty by selecting groups of trees such that the total girth of the trees was approximately the same in each group. For example, in one experiment, Group I. contained 10 trees, varying in girth from 23 to 54 inches, with a total girth of 362 inches; while Group II. contained 10 trees, varying from 24 to 51 inches, with a total girth of 359 inches. But this method should not be pushed too far; it is very unlikely that two trees, 19 and 71 inches in girth respectively, will be equivalent to two others, 40 and 50 inches respectively.

The chief difficulty in the case of *Hevea* is that, although selection by girth is probably the only practicable method, girth is not a reliable criterion of the rubber-producing capacity of the tree. In the Group I., referred to above, only one tree yielded an average of less than 20 c.c. of latex during the first thirty tappings; but in Group II., though the trees, both in individual and in total girth, almost match those of Group I., four trees yielded an average of less than 20 c.c. during the same number of tappings. The results are given in the following table :—

Number of tree	Circumference in inches.	Average daily yield of latex.	Ratio of yield to circumference.
	Group I.		
1	54	54 c.c.	1·00
2	38	27 ,,	0·71
3	20	15 ,,	0·75
4	23	61 ,,	2·65
5	54	63 ,,	1·17
6	27	27 ,,	1·00
7	44	51 ,,	1·16
8	24	48 ,,	2·00
9	48	45 ,,	0·94
10	30	32 ,,	1·07
	Group II.		
1	24	11 c.c.	0·46
2	32	14 ,,	0·44
3	32	16 ,,	0·50
4	51	55 ,,	0·08
5	44	39 ,,	0·89
6	27	16 ,,	0·59
7	26	54 ,,	2·00
8	44	24 ,,	0·55
9	30	24 ,,	0·80
10	39	35 ,,	0·90

Thus, in Group I. the yield of latex per inch of circumference falls below 1 c.c. only in three instances ; but in Group II. it is less than 1 c.c. in eight cases. The rubber-yielding capacity of Group II. is therefore much below that of Group I., though it may not be as bad as it seems since part of the reduction may be due to a diminished rainfall ; for Group I. was tapped daily in June and July, and Group II. every other day in June, July, August. But it is evident, if the results of each group are considered separately, that the yield of latex is not proportional to the girth of the tree.

The only method of avoiding this difficulty would seem to be that all the trees should be tapped in the same way for twelve or twenty tappings, and those which proved to be poor latex producers discarded. After a suitable rest, the remaining trees might then be used for future experiments. But it would be still be necessary to consider girth as well as initial yield, because the smaller trees would be expected to fall off in yield more rapidly than the larger.

It is obviously unfair to compare, say, the yield obtained in daily tapping from trees which are tapped for the first time, with the yield obtained in alternate-day tapping from trees which have already been in tapping for some months. The two groups of trees should start in the same state; they should all have been subjected to the same treatment previously, or preferably they should be untapped trees. Probably further knowledge of *Hevea* will remove this restriction, but it is one which must be insisted on at the present time.

Control Plots.

In determining the effect of any system of cultivation upon the growth or yield of *Hevea*, comparison must be made with another plot—as nearly equal as possible to the treated plot in all respects—which has not received any treatment. The latter is the *control plot*, and without it the experiment is valueless. There is an idea current that the treated plot can be 'compared with itself,' that is, that its yield after treatment can be compared with its yield in previous years; but this is quite a mistake. In *Hevea*, for instance, a manured plot may give a greater yield than it did in previous years; but this might be due to the greater age of the trees, or a more favourable season, or to the treatment adopted; and it is only by comparison with a control plot, on which the first two factors are also operative, that we can determine whether any advantage is to be attributed to the treatment. It is also incorrect to select one plot for the experiment and to take the whole of the rest of the estate as the control, since the conditions over the whole estate may vary enormously; the control should be selected as carefully as the treated plot.

One Thing at a Time.

The most common error in experiments on *Hevea*, and in tropical agriculture in general, is the failure to recognise that if the cause of any observed difference is to be ascertained, only one factor must be varied at a time. If, for example, it is desired to show the difference in the yields obtained by tapping in the morning and in the evening respectively, all the other details of the experiment must be the same for both groups of trees. Each group should contain the same number of trees as nearly equal in girth as is possible; both groups should be tapped on the same day to avoid climatic differences; both should be tapped by the same system and at the same interval, for the same number of tappings; and the same kind of instrument should be employed throughout. Everything should be identical for both groups, except that one is tapped in the morning and the other in the evening. If the two groups are tapped by different systems, on different days, for different numbers of tappings, no one can disentangle the results and attribute any observed difference in yield to any one cause. Probably the commonest mistake of this class is to tap the two groups on different days, although it is well known that the yield is influenced by the rainfall; another common one is to use the knife or pricker at random, according to the fancy of the coolie. But the real cause of such failures is generally a lack of experience in the art of experiment, and a consequent inability to distinguish the factors which may influence the result.

Vacancies. When experiments are made to ascertain the yield per acre under given conditions, it is not permissible to make any allowance for vacancies. The acre plot must be taken as it stands, for the condition of the trees, their size and vigour, is a consequence of the number present on that particular acre. For the same reason, it is not permissible to make any allowance for 'bad trees.'

It is often stated that raising the yield to a standard number of trees, *i.e.*, calculating what the yield would be if there were, say, 200 trees to the acre, gets rid of the effect of bad trees. But it does not, unless one omits those trees altogether in ascertaining the yield. And to go round the plot and decide that this tree shall be counted and that shall not, introduces the personal factor to the fullest extent. Of course, if the experimental plots are attacked by any disease, and numbers of the trees die, the experiment is at an end; one can only begin again elsewhere.

The Duration of an Experiment. This must depend upon the object of the experiment. But in general the experiments hitherto made on *Hevea* have not been carried on long enough. In comparative tests of tapping systems, the experiment should be prolonged for a complete cycle of tappings in each case, *i.e.*, until all the cortex on the lowest six feet of the stem has been tapped; and if it is possible to carry out two complete cycles of one system in the period which only allows one cycle of the other, they should be performed. For example, it is of little value to compare the yields obtained in six months by two different systems, when in one case the trees can only be tapped for one year and in the other they can be tapped for four years. Of course, the complete cycle must include any resting periods which are required.

The Aim of an Experiment. An experimenter must have a clear idea of what point he intends to test, and the experiment must be arranged accordingly. As a rule, he will also have some idea what the result is going to be. It is quite a mistake to imagine that experiments are carried out blindly, and that the experimenter sits down before any result that happens to arrive, in a condition of amazed thankfulness, with a sort of 'fancy meeting you' expression. Presumably he has read what has previously been done on the same subject, and, if so, he must have formed some working theory of the processes involved in his experiment. Such a theory will lead him to expect certain results. If those results really occur, well and good; he may take them as confimation of his theory. But it is probably better when the results do not agree with his preconceived opinion, for then he will examine them further and make other experiments to find where his theory was wrong; and these further experiments may lead to fresh discoveries. Needless to say, he should not allow his preconceived opinions to influence his judgment of the results.

Practical and Theoretical Results. In this connection the term 'theoretical' is employed not to denote results which ought to have been obtained, but those which help to extend our knowledge of the theory of rubber tapping. In distinction to that, a 'practical' result is one which is of value to the planter only. For example, in testing the yield obtained by different systems of tapping it may be correct, from the planter's standpoint, to count only the yield of first-class rubber and to neglect all consideration of scrap or the rubber which may coagulate in the collecting tin. But such results, though of undoubted value to the planter, cannot be taken to show the quantity of rubber in the latex at different seasons, or at different stages in a tapping period, or the difference in composition between the latices from young and old trees respectively. For the latter purposes all the rubber must be taken into account, and any water added to the collecting cups must be subtracted from the measured quantity of water plus latex. Theoretical results, as defined above, must take everything into consideration; practical results may include just as much as the planter thinks they ought to.

Supervision. To write of supervision in connection with experiment is rather an anomaly, but in the tropics, where all the work of tapping is performed by the coolie, the closest supervision is required. It is impossible to place any reliance upon results unless the experiment has been conducted under the immediate and constant supervision of the superintendent or scientist who has planned it. The most explicit directions are neglected, or varied to such an extent that the experiment is worthless; and one can only consider himself fortunate if he happens to discover what has really been done. Further, when the work is entrusted to a conductor, it is of the utmost importance that he should not know what result is expected.

Under this head there must be mentioned another point, viz., the supervision or inspection of the figures which are recorded day by day, or week by week, during the course of an experiment. By such examination various new facts may be discovered which would otherwise be passed over or lost in the total result; and it will also be possible to decide whether the figures necessary to establish any valid result are being tabulated, or whether it is advisable to record other data as well. This is not an imaginary necessity; one instance can be cited in which a series of experiments was carried on for several years without any result, because the really essential figures had never been recorded and could not then be ascertained.

What to Record. Very little credence can be given to an account of an experiment which merely records the final results. We require to know not only the results, but also how they were obtained, and for that reason all the data possible should be published. When the experiment has been conducted properly the yield of each tapping will he known, and if the number of tappings is small each should be recorded; but in a prolonged experiment it is sufficient to group the tappings and give the yields, say, of each ten tappings. But records of each tapping should, in any case, be kept by the experimenter, if he is to deduce much from his work. Very little can be learnt if the rubber is allowed to accumulate and is weighed every month or two months.

Any data which might be of service in enabling a comparison to be made with other experiments should be recorded, especially the age and girth of the trees; the latter are frequently omitted, and consequently the experiment affords only an isolated result; it cannot be correlated with other experiments. The rainfall during a tapping period should always be recorded and published in detail, if possible: many recorded experiments are useless because this has not been done.

Records of yields should give the weight of rubber obtained, not the volume of latex. The variation in the yield of latex is greater than that in the yield of rubber, and hence records of latex only are likely to be misleading. Further, the rubber should always be weighed in the same condition of dryness; and it should be clearly stated whether the figures represent wet rubber or dry rubber.

When it is thought desirable to state that the yield is 'at the rate of so many lbs. per acre per annum,' the actual yields, acreage tapped, and duration of the tapping period, must be recorded; otherwise the statement is wilfully misleading. In one Henaratgoda experiment the yield for the first ten tappings was at the rate of 14 lbs. per annum; yet the trees yielded only 7 lbs. in a year and a half.

Calculations.

Calculation should be avoided as far as possible: and any result which depends chiefly on the manipulation of figures should be regarded with suspicion. There are, indeed, valid methods of calculation which may be applied to experiments, but it is always necessary to examine them carefully to make certain that they do not introduce erroneous assumptions.

One widespread fallacy in calculations applied to *Hevea* experiments is the reduction of the results to the yield per unit area of cortex excised. When this is applied to show the rubber-yielding capacity of two trees, or two regions of the same tree, it assumes that the amount of rubber obtained is proportional to the amount of bark excised. Practically it assumes that the rubber is derived from the piece cut out. But it is readily seen that the latex exudes from the remaining cortex, and it has already been shown that it must be drawn from other parts of the tree. It is generally accepted that the width of the strip removed at one tapping (within limits) has no influence on the quantity of rubber obtained at that tapping. There have been no experiments on that point, but it may be regarded as correct. One tapper may remove only one-twentieth of an inch, another might cut off one-tenth ; but the yield of rubber would be the same in both cases. If the yield were proportional to the area of bark excised, the second tapper should have obtained double the amount of the first. Or, to put it in another way, if A works through a strip of cortex one inch wide in twenty daily tappings, and B works through the same in ten daily tappings, A will get about twice as much rubber as B from the same area of bark. It is the number of tappings which affects the yield, not the amount of bark removed.

Therefore it is only permissible to compare yields by a method which involves the measurement of the amount of cortex excised, when the strips removed per tapping are of the same breadth on all the trees concerned. Examination of the published figures in several experiments shows that this condition is seldom fulfilled. Even when the basal six feet is being tapped, the cuts vary from twelve to eighteen to the inch. When higher parts of the tree are tapped, the strips removed are usually much broader than those removed at the base, as is only to be expected when we remember that the tapper may be swaying about at the top of a fifty-foot ladder. Consequently we must compare the yield for the same number of tappings in order to ascertain the relative productiveness of different trees or different regions of the same tree.

Another fallacious calculation is often applied to the yields obtained by tapping every other day. Two groups are tapped, one every day and the other every alternate day, for a given period. At the end of that time the latter group has received only half as many tappings as the former, and therefore its yield is doubled to ascertain what would be obtained if it were tapped for the same number of tappings as the other. But this calculation is wrong, because it assumes that there will be no diminution in yield as tapping is continued. In actual fact, it is known that the second half of the tappings will not yield as much as the first.

We have advanced beyond the stage at which it was believed that the yield per acre could be estimated by ascertaining the yield of one tree and multiplying by 200, but a somewhat similar idea is still in existence, viz., that all yields should be calculated to a standard number of trees per acre. If, for example, a measured acre contains only 160 trees, then the yield obtained is increased by 25 per cent. to show what it would have been if there had been 200 trees to the acre. This idea has long since been abandoned in countries where agricultural experiment is more advanced, for it is recognised that the condition of the trees—their roots, stems, and crowns—is a consequence of their distance apart, and that it would not be the same if there were a greater number to the acre. So long as this idea persists it must be insisted that the actual yields of the plots be published, as well as the calculated yields, and that the latter should be unmistakably labelled 'calculated.'

Attention may be directed to a point in arithmetic which is commonly misunderstood. It is frequently argued that when water has been added to the collecting cups it is a waste of time to subtract the amount added from the total fluid collected in order to ascertain the yield of latex, because the amount added is the same for each tapping. But as the yield of latex is not always the same, the addition of a constant amount of water does make a difference in the result of the experiment. Suppose, for example, that the yield of latex was 100 c.c. and that 20 c.c. of water were added to the cups, then the percentage of rubber ln the latex would be reduced by 16 per cent.; but when the yield increased to 300 c.c. the same quantity of water would only reduce the percentage of rubber by about 6 per cent. Adding the same quantity to the two terms of a ratio alters the value of that ratio; and therefore, since all our comparative experiments deal with ratios, the inclusion of a constant quantity in each case alters the result.

In some instances, yet another inadmissible calculation has been introduced. Groups of trees containing equal numbers are selected for experiment, and tapping is carried on by different methods for several months. Subsequently it is found that some of the trees are not yielding much latex, and these are 'rested,' different numbers of trees being 'rested' in the different groups and at different times; and, on the supposition that it will neutralise this evident defect, the numbers of trees tapped are averaged to obtain the final result. For example, if a group contains 50 trees, all of them may be tapped during the first month, 40 for the second month, and 30 (possibly including some of those previously omitted) for the third; and in tabulating the results, they are reckoned as 40 trees tapped for three months. In a parallel group, the corresponding numbers may be 50, 47, 44, which would be reckoned as 47 trees tapped for three months. If the object of the experiment is to compare the yields obtained by different systems, this calculation is inadmissible, for the condition of the trees is a result of the system. The trees must be taken as 50 in each case, even though some of them have been 'out of tapping' for part of the time. It will be evident that by the above method of calculation the system which inflicts the least injury on the tree (*i.e.*, which puts the tree 'out of tapping' for the least number of times) is made to appear the worst as far as yield per tree is concerned; and when the original number of trees selected for the experiment is not stated, the calculated results are likely to be highly misleading. Similar remarks apply to the system of averaging the trees to a standard number of tappings.

Discussion of Results.

When an experiment is concluded it is incumbent upon the author of it to examine all the data collected and deduce what conclusions he can from them. He only knows what has been done and how far the records may be taken to prove anything. The figures must be considered in every possible aspect and any probable flaws pointed out. Not only should he record what conclusions may reasonably be drawn, but he should also point out and give reasons for rejecting any deduction which, though apparently warranted by the data, is really unsound. This entails a considerable expenditure of labour and thought; but if the experiment was worth doing, it is surely worth while to get all the information possible out of it. It is a waste of time, both the experimenter's and the readers', to publish a mass of undigested figures.

Experimental Error.

If two plots of land, apparently equal in every respect, are selected for experiment, it will generally be found that although they are cropped, manured, and worked in exactly the same way, they will not produce the same yield. Plot A may sometimes be better than Plot B, as judged by the annual yield, and sometimes worse. But if the experiment is continued for a number of years one plot will be found to be, on the average, superior to the other. Thus we find that not only are two experimental plots rarely identical, but that the yield of the two plots in a single year may not show their true relative position. These facts must be taken into account in estimating the value of the result of an experiment.

From an examination of a large number of experimental results, A. D. Hall, the director of the Rothamstead Station, concludes that the mean error to be attached to the yield of a single plot is generally about ten per cent., that is, the yield may be either ten per cent. above or ten per cent. below what it should have been. 'In other words, if we have three experimental plots giving yields of 91, 100, and 109 respectively in any one year, it is not right to conclude that such differences have been brought about by the treatment; the three plots must be considered as giving equal results.' It follows that if a single experiment only shows a difference of less than twenty per cent. between two plots the result is inconclusive. 'The only way of reducing this "experimental error" and obtaining a closer result is to multiply the experiments, either by repeating them year by year or by increasing the number of plots, preferably both, because there may be constant differences in the soil, while the season also may induce variations in the effect of the treatment. Increased accuracy cannot be obtained by increasing the size of the plots.'

Measurable Quantities. It is impossible to measure any quantity absolutely correctly. If the same length be measured, or the same quantity weighed, several times, with the greatest accuracy possible, the lengths, or weights, obtained will differ by small amounts. Every measurement involves some error, but the permissible error depends upon the object in view. A surveyor may be content to measure distances correct to the nearest foot, but a carpenter may only allow himself an error of one-eighth of an inch, while a laboratory worker may desire to be correct to a thousandth part of an inch. Much time is wasted in recording measurements which cannot possibly be correct to the figures stated. For instance, it is impossible, with the methods ordinarily in use, to measure the girth of a tree correct to one-eighth of an inch; the nearest quarter-inch is all that can be expected, and the girths should be recorded in inches and quarter-inches. When the girth of a tree is measured at a given height from the ground at intervals of one or two years, measurements to an eighth of an inch are still more to be avoided, for there is every probability that the tree is not measured at the same spot on successive occasions. The following example may be given. Several groups of trees were measured and their girths recorded; two years later they were measured again; and the measurements were repeated after a further interval of two months. In one group the increase in girth in two months was equal to that in the previous two years; and in another the increase for two months was three times that for the previous two years. The truth was that the differences were so small that they were less than the errors made in measuring.

The foregoing remarks are not intended to convert every planter into an experimenter. There is an idea that any one who has a plot of land at his disposal can immediately begin experiments for himself, but the experiments which he can attempt are very limited, and it is not an easy matter to decide what they are. The art of experiment has advanced considerably, even in agriculture, and the day when any one, without previous training or a study of the literature of the subject, could take a hand in it has gone by. One of the founders of electrical science made his discoveries by the aid of a frog's leg and two pieces of metal, but no one would now expect to advance wireless telegraphy or electric traction by experimenting with the same materials and with the same standard of knowledge.

The main object in setting forth what details must be observed if experiments are to prove anything, was to enable the planter to criticise such experiments as may be published for his edification. Every experimenter who publishes his results invites criticism, and it is only by the help of such criticism that progress is secured.

A conducts certain experiments, and gives his results to the world at large; if B, on reading A's account, finds that certain material factors have not been taken into consideration, he says so. Perhaps he will repeat the experiment, and either modify or confirm A's conclusions; and this process is continued until some acknowledged truth ultimately emerges. It is quite a mistake to suppose that the result of one experiment is necessarily correct; and even the repetition of an experiment does not always secure accuracy. The discovery of argon is a case in point. Experiments demonstrating the composition of the air had been repeated for nearly a century, but it was only comparatively recently that it was proved that all these experiments had been inaccurate, and that the air contained other gases which had not previously been recognised. In temperate climates agricultural experiments are criticised as freely as those of pure science; consequently, agriculture is advanced, and the public is protected from false deductions. Recent instances of the latter will be recalled by any one who is conversant with agricultural literature. But what passes for experiment in the Tropics is, in the majority of cases, valueless; and it does not improve because of the lack of efficient criticism.

CHAPTER VIII.

General Sanitation.

In olden times diseases of plants were regarded as 'visitations,' and allowed to rage unchecked. In seasons favourable to their development, their ravages caused widespread damage and, in some instances, the almost total ruin of a country; and only the advent of less favourable years brought relief. Even after it had been proved that plant diseases were due to the action of specific organisms and that some steps could be taken to combat them, little improvement resulted at first, because, in the majority of cases, nothing was done until the disease had obtained so strong a footing that nothing could be done successfully at any reasonable cost. The fungus of coffee leaf disease (*Hemileia vastatrix*) was recognised to be a very destructive pest in 1869, but it was not until ten years later that active measures were taken against it. There is little doubt that much of this delay was due to a reluctance to admit that any disease existed; and it is only within the present century that public opinion in planting countries has come to understand that plant diseases are as inevitable as those of men and animals. Less than ten years ago the issue of a circular on a disease of tea brought letters, either abusive or supplicatory, pointing out the supposed injury which such publication inflicted upon the industry; but at the present day similar circulars may be published every month without exciting any such response.

This acknowledgement of the inevitability of disease leads immediately to a recognition of the fact that it is necessary to be always on the alert to observe any abnormal or suspicious appearances, and to have inquiry made into them at the earliest possible moment. Speed is an essential factor in the treatment of diseases, and to deal with any one of them successfully it must be attacked in an early stage. In *Hevea*, at least, this proposition is thoroughly understood; and there is little fear that any disease will be allowed to proceed unchecked or unobserved in Eastern plantations, so long as the present vigilance of estate superintendents is maintained. Of course, this implies that the observer has a full knowledge of what is normal in *Hevea*, and it is hoped that what has previously been written will assist him in obtaining that knowledge. At present many trees are sacrificed unnecessarily because the planter thinks they *may* be diseased, and sends them in for examination: still, this is erring on the right side.

But though the recent advance of public opinion in this respect has been extraordinarily rapid, it stills falls short of what is absolutely necessary. The continued study of plant diseases has shown what conditions are favourable to their development, and consequently what precautions should be observed if they are to be avoided or minimised as far as is humanly possible. In short, such knowledge enables us to advance from the idea of remedial measures to that of preventive measures. It is no longer permissible to adopt systems of planting or methods of cultivation without considering their probable effect when diseases arise; and in the light of our present knowledge, that effect can in many cases be predicted with a close approximation to certainty. The pathologist should be consulted beforehand, not five or six years afterwards when some disease has already appeared: and in the absence of any such consultation he would fail in his duty, if he did not point out how new or old planting practices tended to promote disease.

Furthermore, in addition to preventive measures which must date from the opening up of the estate, there are many details of general sanitation which should be attended to if it is desired to keep the trees in a healthy condition. Some of these are indicated in the present chapter.

Jungle Stumps.

By the usual method of clearing jungle land for planting in the tropics, all the stumps of all the trees are left *in situ*. That is a fact which agricultural experts and inventors in temperate climates find some difficulty in realising. In temperate countries the trees are felled; and the stumps are afterwards extracted because the land is to be worked by machinery; but in the tropics machinery is not employed, and therefore this necessity does not exist. Further, tropical trees, especially on low-lying land, or in 'rain forest,' are often furnished with high buttress roots, and to economise labour they are cut above the latter. Thus, not only are stumps left to decay by natural means, but they are larger and more numerous than in temperate countries.

The decay of these stumps is brought about by the agency of fungi, the spores of which alight upon the exposed wood and germinate there. The fungus threads (hyphæ) attack the wood, and either gradually consume it or else absorb certain parts of it so that the remainder falls into powder. In either case the fungus feeds unseen upon the tissues of the stump, and in due course constructs fructifications of varied form and colour on the exterior of it. The majority of these fungi are merely saprophytic, *i.e.*, they can live only on dead tissues, but some of them can act as parasites on occasion, and it is the latter which cause trouble. All the root diseases of *Hevea*, tea, and cacao which have been investigated with any approach to completeness have been found to originate on a neighbouring stump; in some cases it is the stump of a jungle tree, while in others it is the stump of a tree which has been planted for shade and then cut down. But there is no known root disease of any of the plants mentioned which attacks the plant directly, *i.e.*, by the germination of spores upon the plant; they all require an external base of operations, and this they find in the dead wood of an adjacent stump.

The general plan of attack is as follows. The spores of the fungus are blown on to the exposed wood of the stump, and if the weather conditions are favourable they germinate and their hyphæ grow down into it. These hyphæ continue growing in the dead tissues until they have permeated both the stem and the roots, and then they spread from the roots of the stump to the roots of adjacent living trees. Some fungi can only spread to other plants if the roots of the latter are in contact with those of the host stump; others, however, can spread freely through the soil, drawing food from the supply in the stump which served as a base. Each stump thus affords a centre of disease, spreading destruction in an ever-widening circle.

In addition to spreading the disease by means of radiating fungus hyphæ in the soil, each infected stump produces fructifications of the fungus, and these liberate spores which convey infection to other stumps. In some cases fructifications are produced at intervals from shortly after the stump is first attacked until the time when it is completely decayed; while in the case of other fungi the stump only bears fructifications when it is in the last stages of decay.

If there were no dead stumps there would be no root diseases either in *Hevea* or tea. But it is not an easy matter to get rid of them, and whatever method is adopted the cost is high. They have, however, been got rid of in certain cases, both in Ceylon and Malaya. In 1906 I recommended that course in dealing with *Fomes semitostus*, and on one affected estate in Ceylon all the stumps were dug out. Several estates have since adopted the same treatment in Malaya, while others are only deterred by lack of funds.

Owing to the long period which must elapse before any return can be obtained, an estate must be planted up as soon as possible after felling and burning off. Hence, apart from other reasons, such as loss of soil, &c., it is idle to expect that the land will be cleared of stumps before the rubber is planted. Stump-extraction must follow planting in ordinary estate routine. But here we are met with an insurmountable difficulty. Any stump extracting apparatus capable of dealing with jungle stumps cannot be employed after the estate has been planted up without causing an enormous amount of damage; and those which can be used with comparative safety are of very little value. As a rule, inventors of stump-extracting machinery under-estimate the difficulty of the problem with which they have to deal, owing to entire absence of any firsthand acquaintance with local conditions. Because of the importance of this question a medal was offered at the Rubber Exhibition of 1906 for the best method or apparatus for extracting stumps; but only one entry was received, and that was not considered worthy of any award. At the present time nothing more can be recommended than digging out the stumps to a depth of two or three feet by manual labour; and that operations should be begun as soon as the plants are established.

It has recently been proposed that, in order to avoid the danger of root diseases, estates should be planted, when first opened, not with the product which it is intended to cultivate permanently, but with some other plant which can be grown for three or four years with a reasonable prospect of profit, and then cut out. In this way the jungle stumps would be given time to decay, and any root diseases which might arise would attack (?) only the temporary crop. These diseases could then be dealt with as drastically as wished, without any hesitation on the ground of permanent loss. The land would then be clear of disease and could be planted up permanently.

The above method will scarcely recommend itself to the rubber planter of the present day, who wishes to see some return for his money as soon as possible, and he will no doubt continue to take the risk of root disease rather than add four or six years to his enforced waiting period. But the proposal immediately suggests the question, what length of time elapses before jungle stumps decay? This question does not admit of a definite answer, since the time of decay depends upon the kind of stump, the elevation of the district, and, to a great extent, upon the weather; and if the stumps are attacked by white ants their duration is considerably briefer than would be the case under other conditions. But the data available indicate that the time required is much longer than is commonly supposed. It would appear that, given the maximum amount of assistance by white ants, the stumps of soft-wood trees may decay fairly completely in about four years in a wet, low country district. On the other hand, hard-wood stumps (*e.g.*, jak) show very little decay after seven years in a wet district at an elevation of 1500 ft. (Ceylon). As an extreme case, the following may be cited: On stumps of 'Na' (*Mesua ferrea*), subject to a rainfall of 200 inches per annum at an elevation of 1500 feet, only the exterior sap-wood had decayed at the end of fourteen years, the heart-wood being quite sound and likely to persist for at least as long again. Unfortunately, most of our jungle trees are hard-wood trees, and therefore they are not likely to decay completely in six years.

This suggested method is really an extension of the well-known 'trap-crop' method often recommended in dealing with eelworms in crops of short duration. Some crop which is known to be susceptible to their attacks is sown on the affected soil, and after a few weeks the plants are uprooted and burnt, most of the parasites being removed with them. In the present case it is proposed to 'trap-crop' with coffee, but unless the extraction of stumps was proceeded with at the same time it is doubtful whether much benefit would be obtained in the period allotted. Moreover, unless the fungi known to attack *Hevea* also attacked coffee, there would be no indication of their presence, and the land might be planted up with the former just at the time when the fungi were most prevalent. As far as is known at present, *Fomes semitostus* and *Sphærostilbe repens* attack *Hevea*, but not coffee ; while *Hymenochæte noxia* attacks both.

At the present time both the tea planter and the rubber planter 'trap-crop' with their main crop. In the case of tea the young plants may be attacked and die at a fairly early stage, but in *Hevea*, unfortunately, little is visible before the trees are two to four years old. In spite of that, however, this method is to be preferred to that of trap-cropping with coffee for four years, because the surviving trees are approaching the tappable stage by that time. But if root disease is to be avoided the removal of stumps should be begun as soon as the estate has been planted up.

It has been suggested that the stumps should be treated with some chemical which would soak into them and protect them from the attacks of fungi. But unless this process were repeated periodically it would merely postpone the evil day, because the fungicide would leach out again and leave the stump liable to the attacks of fungi when the plantation was older. The effect of root disease is soon evident on a young tree, but it might spread to a considerable distance from an old tree before its effect on that tree was observable. The Australian method of burning out stumps has also been recommended. In that process several holes are bored in the stump, and these are filled with saltpetre and water ; when the stump is dry kerosene is poured into the holes and set on fire. Stumps never burn away completely by that method, and much labour is necessary to cut away the remainder. Gallagher has recorded the following method of burning stumps, which has been found successful in Perak. 'The earth is cleared away to a depth of three feet round the root of the stump and a distance of three feet from it, leaving the roots exposed. Timber is then cut up and put into the trench to the depth of about one foot, then a layer of burning charcoal sufficient to ignite the wood. This is again covered with grass or bark, and the whole is covered with the earth taken out of the trench, which must be firmly pressed down. Care must be taken that all the timber thrown into the pits is well covered with earth. The fire will continue for weeks until the roots, and ultimately the whole stump, gradually burn away.'

At the annual meeting of the Pataling Rubber Estates Company, in April 1910, it was stated that the expense of uprooting stumps and removing all dead wood came to a total charge, 'once and for all,' of less than sixpence for each rubber tree; 'that is not a very heavy insurance to pay to rid the trees of what may cause a great deal of injury.'

Planting Distances.

During the last three years opinions on this subject have changed to the view which had to be fought for in 1905-6, viz., that if a *Hevea* plantation is to be remunerative for any considerable time, the trees must be planted far enough apart to allow them to develop normally. In 1905, *Hevea* was being planted, 8 ft. by 8 ft., 10 ft. by 10 ft., or 12 ft. by 12 ft. To any one with a knowledge of plant diseases it was evident that such planting was too close, but it was not until the Rubber Exhibition of 1906 that an official declaration was obtained in favour of 20 ft. by 15 ft. as an average distance. It is now recognised by many planters that even that distance is too close for mature trees, and some consider that thirty feet should be allowed when the trees are old. Such an opinion would have been derided in 1905, but once having accepted a reduction from 680 to 140 trees per acre, further steps in the same direction are comparatively easy.

It is, no doubt, difficult for any one to realise to what extent the system of planting may favour the development of fungi, unless he has had a fairly long experience of actually searching for fungi in the field. Then he learns to direct his steps to places where the shade is dense, and consequently the humidity high; or he seeks the northern side of a wood, where the sunlight seldom falls, in preference to any other. This variation according to local conditions is strikingly exhibited by our tropical cultivations. A coconut estate, even in a wet district, is almost barren ground for a mycologist, because the trees are planted so wide apart that their crowns afford little shade, and the free circulation of air between their stems prevents undue humidity. A tea estate is not much better, for although the bushes are closely planted, they are not high enough to retain an abnormally moist atmosphere. But a cacao estate is a mycological paradise; the trees are so densely planted that the sunlight never penetrates between them, and they are so tall that the air near the ground is maintained constantly at a higher degree of humidity than elsewhere.

The effect of thinning out cacao and removing shade trees was well illustrated on the Experiment Station, Gangaruwa, during 1902–05. The estate was taken over in May 1902, when the cacao was badly diseased and the yield had decreased from 2·86 cwt. per acre in 1897–8 to 0·62 cwt. in 1900–01. Ninety-six per cent. of the trees were diseased. 'The total acreage under cacao was 150, and this contained no less than 76,193 plants, over 3 ft. high, equivalent to 508 plants per acre, or planted at an average distance of 9 ft. apart. When one realises that no less than 8959 trees, including the colossal *Albizzia moluccana*, *Bombax malabaricum*, jak, and sapu trees existed, there can be no difficulty in realising what a happy nest the cacao fungus had' (Wright, February 1903).

From 1902 to 1905 the shade trees were cut out, the cankered bark regularly excised, and diseased pods collected. The following table gives the number of cacao and other trees per acre, the percentage of diseased pods, the yield of cacao per acre, &c., from 1902 to 1906 :—

	Cacao trees per acre.	Other trees per acre.	Percentage of fungus pods.	Yield per acre.	Rainfall.	Cost of excision work per acre.
				Cwt.	Inches.	Rs. c.
1902 ...	330	178	38·6	0·83	(111·56)	11 43
1903 ...	252	77	8·8	1·18	(71·83)	17 39
1904 ...	246	71	4·8	2·06	105·35	12 15
1905 ...	328	440	2·5	3·57	78·74	9 28
1906 ...	330	450	10·2	2·43	79·97	3 2

It will be seen that the number of trees per acre, especially the shade trees, was steadily reduced during the first three years. In the fourth year (1905) there was an increase both in the number of cacao and the number of shade trees ; the former was due to supplies which attained a height of three feet in 1905, and the latter to dadaps, which were planted in 1904 and would be only small in 1905. The yield given for 1902 is that for May–December only, and the cost of canker excision covers the same period. The percentage of fungus pods decreases steadily down to 1905. In 1906 it increased to four times that of the previous year, in spite of the fact that the whole estate was sprayed in 1905 and 1906. There is nothing in the rainfall which would account for this increase, and it cannot be attributed to anything but an increase of shade, the estate having then become overgrown with dadaps. This case illustrates admirably how alterations in the amount of shade affect the progress of a disease; and, though it does not relate to *Hevea*, it is relevant to the present discussion, because the fungus of cacao pod disease and canker is identical with that of *Hevea* canker.

It has been argued that, since fungus spores blow everywhere, it can make little difference whether the trees are widely or closely planted. For example, it has been stated that 'It may even be disputed whether the differences in distance between widely and closely planted trees of Para rubber is an effective check against the spread of many diseases, especially where leaf pests are concerned. Distance does not give immunity from attack on an ordinary rubber estate; the differences under discussion are trivial when one considers how spores and insect pests may travel.' But as far as fungi are concerned, this objection misses the chief point. It is not a question of whether spores will arrive at any given spot, but whether they will germinate when they get there. To ensure germination they require moisture and shade; if they are kept dry they will not germinate, and if they are exposed to sunlight they are killed. The conditions on a closely planted estate are much more favourable both for germination and growth than on one widely planted.

Closely planted trees are ultimately tall and thin, and only slightly branched. Their lower branches are killed by the dense shade, and their crowns consist of only a few branches directed more or less vertically and bearing comparatively few leaves. The effect of this on the growth and bark renewal of the trees has already been described, and need not be repeated here; but there is another effect which is not generally recognised, viz., that the crown affords no protection to the lower part of the stem. Though the canopy of leaves is sufficient to keep out the sunlight, it offers no protection from the rain, which beats straight down through the trees, and streams down the stems, while the almost vertical upper branches direct the water along the same course instead of away from the stem. Hence in very wet weather it is sometimes impossible to tap, because the latex flows over the wet bark instead of along the tapping cut. Moreover, this continuous current of water down the stem favours infection by the canker fungus, and also induces decay of the renewing bark quite independently of any parasitic organism. The advantages of spreading branches were conspicuously demonstrated in a recent attempt to infect certain Peradeniya trees with 'canker.' Although the experiment was conducted during the monsoon, it was found that their stems remained quite dry during the rains, owing to the protection afforded by their leafy crowns, and in order to imitate the usual estate conditions, special arrangements had to be provided to keep the inoculated stems damp.

It is generally admitted that closely-planted areas yield more rubber per acre during the first few years than those widely planted, though comparative experiments on this point are lacking. The report of the Bukit Rajah Company for 1909 stated that the best yielding fields were those planted 40 ft. by 40 ft., and inter-planted so as to make them 27 ft. by 27 ft.; the yield from one of these fields was 4 lbs. per tree, or 300 lbs. per acre, but the age of the trees was not stated. It was also recorded in the same report that 'the fields planted 20 ft. by 10 ft. in 1906 have the tops touching at 20 ft. apart; 21 ft. by 21 ft., at five years old, the tops are interlaced. In the crowded fields the bark does not renew so thickly, or the trees yield so much latex as in the widely-planted fields.'

The effect of close planting upon the duration of the plantation must also be considered. A block of trees at Henaratgoda, now over twenty years old, is planted 12 ft. by 12 ft. Some of these trees were tapped for a year during 1905–06; and in 1908 further tapping experiments were begun, the trees being selected both from the tapped and untapped trees of 1906. Thus in 1910 some had been tapped once, others had been tapped twice, while others had never been tapped. Yet the growth of these trees is practically at a standstill, and Lock and Bamber stated that they were beginning to show obvious signs of the ill-effects of close planting in 1908, although most of them had never been tapped.

It is said that *Hevea* may be planted closer on hillsides than on the flat, presumably because the slope raises the crown of each tree above that of the tree immediately below it, and thus diminishes the amount of interference between them. But it is forgotten that the slope automatically brings the trees nearer together. To take an extreme case, if the slope of the hill is 60°, and the trees are planted 20 ft. apart along the slope, they are only 10 ft. apart horizontally; if the slope is 30°, they are 17⅓ ft. apart horizontally. If anything, therefore, the trees should be further apart on sloping ground, provided that the growth is equal.

Thinning Out.

Close planting with the intention of thinning out in later years was never widely adopted. In the majority of cases, estates were planted up at the distances it was considered the trees would remain permanently, and it is only of recent years that it has been realised that even with the wider distances some thinning out will have to be done when the trees interfere with one another. It is impossible to fix any definite age at which the trees should be thinned out—that will depend upon the distance, the quality of the soil, and other local conditions—but the operation should not be delayed too long, or the remaining trees will make little response to the altered conditions. It would not be of much use to thin out a closely planted field, after the crowns had been reduced to a few vertical branches, in the expectation that the remaining trees would develop lateral branches lower down; improvement will as a rule be limited to the existing crown. The trees should be so far apart at the beginning that they can spread out their crowns without interference and build up a normal framework of branches; where this was not the case—as, for example, where trees were planted 8 ft. by 8 ft.—little alteration can be effected in the shape of the tree by thinning out after the age at which tapping has begun.

When it has been decided to take out a tree it should be removed as soon as possible. If it has been regularly tapped for some years it should be removed at once. Whenever it is destroyed it will contain some rubber, and the planter might as well make up his mind to lose that at the beginning as run the risk of injuring other trees by tapping it until it is in a moribund condition. If it is desired to obtain as much rubber as possible from it in a short time it should be tapped daily by a half herring-bone on both sides of the tree for about three months; in that time the yield will have fallen to a negligible quantity, and it is not so long that the condition of the tree will be noticeably affected by the tapping. The tree must be uprooted, and all the woody parts taken away; as a rule, trees which are cut out will not be large, and there should therefore be no difficulty in this. If the trees are felled, and the stumps left in the ground, no injurious effects will be immediately evident; for some years the stumps put out new shoots, which are either killed by the shade or eaten off by animals. But experience in Ceylon has shown that trouble begins about five years after the felling, by which time many of the *Hevea* stumps are dead, and have become centres of root diseases.

Intercrops, Cover Plants, &c.

The idea of an intercrop, *i.e.*, a product which could be grown permanently between the lines of *Hevea*, or one which might be grown during the first few years only, so that some return would be obtained before the *Hevea* came into bearing, was never widely adopted, and has now been practically abandoned in the East. Much ink and paper was expended on this subject in 1906, but practical applications of the methods then advised have not been numerous. As a rule *Hevea* estates have grown *Hevea* only, though in some cases *Cassava*, cacao, or coffee, have been interplanted. Of course, on many estates *Hevea* has been planted among existing cacao or tea; this is not strictly interplanting, but a gradual replacement of the previous product by *Hevea*, and the method is dictated by necessity, not by choice. In general it may be said that the rubber planter has concluded that intercrops are not worth the trouble from the financial point of view. They have also been recommended on the ground that they will hinder the spread of disease. That side of the question will be considered later; but, as advised or practised up to the present, intercrops tend rather to promote the spread of disease than otherwise.

From a purely mycological standpoint there are two points to be considered when selecting an intercrop, or a cover plant, for *Hevea* plantations—viz., its effect on the general hygienic conditions of the estate, and the diseases to which it is liable. It is admittedly wrong to select a plant which is subject to the same diseases as *Hevea*, and it is also wrong to select one which will grow so tall and dense that the bottom shade, and consequent humidity, are thereby unduly increased. An intercrop which would grow no higher than tea is usually allowed to grow would be ideal, while a cover plant should, if possible, be lower still.

It is unfortunate that cacao should have been selected as an intercrop for *Hevea*. When established, the shade and humidity in the mixed plantation are greater than if *Hevea* had been planted throughout instead of cacao, and consequently the general sanitary condition of the whole estate is lowered. In addition to that, the diseases of the two plants are in many cases identical. *Hymenochæte noxia*, the cause of 'brown root disease,' attacks both *Hevea* and cacao; *Botryodiplodia theobromæ*, which is the chief agent in *Hevea* 'dieback,' causes dieback in cacao also, and is abundant on decaying cacao pod walls; and *Phytophthora Faberi*, which is the cause of cacao pod disease and stem 'canker' similarly produces pod disease and stem 'canker' in *Hevea*. Taking all things into consideration, it must be concluded that, from a mycological point of view, cacao is the worst possible intercrop which could have been chosen to plant among *Hevea*.

Coffee has been interplanted on several estates in Malaya, and a few in Ceylon. Provided that only one row of coffee is planted between the rows of *Hevea*, the shade is not as dense as in the case of cacao, and from that point of view coffee is to be preferred. The diseases which coffee shares with *Hevea* are 'brown root disease' (*Hymenochæte noxia*) and 'pink disease' (*Corticium salmonicolor*). The latter is likely to be prevalent if the coffee is thickly planted.

Where cover crops or green manure plants have been adopted, the first choice has usually rested with *Crotalaria striata*. This is generally sown thickly, and in most cases it does not grow higher than two to three feet. It suffers from two leaf diseases, neither of which is likely to attack *Hevea*. One of these, which takes the form of circular, dry, brown spots, often concentrically zoned, is caused by *Sphærella crotalariæ*. The other is caused by *Parodiella perisporioides*, a well-known parasite of leguminous plants in the tropics; the fungus forms minute black points scattered over the upper surface of the leaf, but the leaf is not killed—it simply curls up. This latter disease is most prevalent on old *Crotalaria*, after it has been cut back, and on self-sown plants. There is, however, a danger in *Crotalaria* in some districts, where it grows much taller and stouter than it does in Ceylon, *e.g.*, in some parts of Southern India. In these localities it may grow to a height of nine feet in a year without flowering, and may acquire a woody stem about an inch in diameter; such plants are attacked by *Corticium salmonicolor*, and the disease soon spreads to the *Hevea*. There is no danger in growing *Crotalaria* among *Hevea* in most countries; and where the growth is so vigorous that it forms a tall jungle some smaller green manure and cover plant must be adopted, or the *Crotalaria* must be cut down earlier.

From a mycological standpoint any green manure plant which grows tall should not be planted in dense masses; the lower the plant the less is the danger of disease. A plant which would not exceed a foot in height would be ideal and could be sown as thickly as wished. Further, there is always a tendency to grow green manure plants too long. In temperate climates such a crop is often ploughed in at the end of a month; but in the tropics the idea always appears to be to make it run as long as possible, and to obtain some profit by selling seed. There is little advantage in a green manure plant, as such, until it is cut down and mulched in.

Dadaps (*Erythrina sp.*) have in some instances been interplanted among *Hevea* as green manure plants. It was formerly stated that cacao canker (which is identical with *Hevea* canker), attacked dadaps, but it is most probable that the statement is incorrect. Layering the dadaps, *i.e.*, cutting the stem half through and bending it horizontally so that it throws up clusters of new stems, is objectionable, because it produces a dense growth which favours the introduction of *Corticium salmonicolor*.

Although the point deals with animal, not vegetable, pathology, it may be noted that the sensitive plant, *Mimosa pudica*, is unsuited for use as a cover plant because of its thorns. Coolies refuse to walk among it unless they are furnished with some protective covering for their feet; and consequently, on several estates on which it was planted a few years ago thousands of rupees are now being expended in attempts to eradicate it. On coconut estates it can be kept down by goats.

When the intercrop has ceased to be remunerative, it should be removed completely, not allowed to die out and decay *in situ*. Moreover, it is not sufficient merely to cut the plant down; the stumps must be extracted as well. This is especially necessary in the case of cacao, since it has already been demonstrated, on estates where alternate lines of cacao have been *cut out* to make room for *Hevea*, that the cacao stumps serve as centres for 'brown root disease.' Similarly dadaps, albizzias, and tea are attacked, when dying, by *Botryodoplodia theobromæ*, which is already known to attack *Hevea* under certain conditions. In one instance, where tea under *Hevea* was allowed to die out, several of the old *Hevea* trees were killed by root disease, and on one of these the fructifications of *Ustulina zonata* developed; this fungus is the cause of the commonest tea root disease, and is known to develop on *Albizzia* stumps, but it is not yet certain that it causes root disease in *Hevea*. But it will be safer to assume for the present that it may do so, and to take the example last quoted as an additional reason for uprooting abandoned tea under *Hevea*.

Protective Belts.

It is admitted that massing together a large number of plants of the same kind favours the spread of disease, should any disease arise. This condition is especially prevalent in the tropics—much more than in temperate countries where crops are of short duration, and rotation of crops is practised. A certain district is found suitable for some particular product, and the whole of it is immediately planted up with that product. In Ceylon there are about 400,000 acres of tea, nearly the whole in one continuous sheet. Similarly districts suitable for rubber (*Hevea*) are planted up with rubber everywhere.

Two methods of minimising the danger have been suggested, viz., interplanting and protective belts. The advantage of the first method is doubtful, except in so far as the interplanting reduces the shade and humidity over the whole estate; an intervening line of another product cannot do much to prevent the spread of fungus spores, but it may make the cultivation drier, if the plants are properly selected and spaced. What is really wanted is varied cultivation in each district, *i.e.*, a series of adjacent fields or estates of different products, and that is utterly improbable. Simple interplanting merely provides that if one product is destroyed by disease the other may be left; it is obedience to the aphorism that all one's eggs should not be put in the same basket. But it is a difficult matter to find two products which can be grown successfully in the same district, and are not attacked by the same diseases, or do not interfere unduly with one another. Regarded in this light, interplanting is an insurance against loss through disease, not a method of disease prevention; and one must make sacrifices (in the matter of suitability of a district for a particular crop) as payment for the insurance.

But though single lines of trees are of no avail in checking the spread of a disease, broad belts are; and it would undoubtedly be an advantage if the acreage under rubber were broken up into small blocks by belts of other products. It is not necessary that these secondary crops should be taller or denser than the *Hevea;* even if they are quite low they will afford some protection provided that the belt is not too narrow. In this way a diversified cultivation would be obtained. It has been suggested that large estates should be divided by belts in this way, and that they should be completely surrounded by similar belts which would cut them off from neighbouring estates.

At present there is no probability that this suggestion will be adopted. Tea and cacao have both survived without any such precautions—neither having been attacked by any serious leaf disease—and these two examples are allowed to outweigh that of coffee. Moreover, it is out of the question to expect the planter, when he has bought land suitable for rubber at a high price, to plant up a great part of it with some product which is scarcely remunerative; and as estate land is sold at present, an efficient boundary belt would, in the case of a long narrow strip, leave nothing to be planted with the main product, for, to be effective, a protective belt should be at least 220 yards wide. No suitable crop has yet been suggested for such belts. Apart from the fact that the best rubber districts will not grow cacao, the diseases of the latter preclude its adoption. Coconuts and cotton have been suggested; but the latter is clearly impossible, if the district is suitable for rubber, and, in Ceylon at least, the coconut planter has never displayed any inclination to establish plantations in what are now the rubber districts. There seems no other solution than that belts should consist of forest trees from which no return is to be expected.

All this leads to the conclusion that if protective belts are considered necessary, they should be provided for before any land is sold. When a new area is opened up on a large scale, it should be treated as a whole and divided up into suitable sections, by the reservation of belts of forest at definite intervals. The process should resemble the opening of an estate for building purposes, when the whole estate is roaded and drained before the plots are offered for sale. The cost of the protective belts could then be made to fall upon all the owners of the district.

In the Federated Malay States a belt of jungle, sixteen miles long and two miles wide, has been reserved, dividing the rubber district into two large areas. This is the only recorded instance of the adoption of a protective belt. A similar policy was found impossible in Ceylon, because, *inter alia*, the land required was not in the hands of the Government. It is evident that such a course is only practicable when the district is first opened up and can be considered as a whole ; and in most countries at the present time the idea must be regarded as Utopian.

Pruning.

On many estates lateral branches, which arose from the lowest six feet or so of the stem, have been cut off ; or when the trees forked near the ground level one stem has been removed. In the majority of cases the branch or stem has been sawn across a few inches from the main stem, thus leaving a 'stub' two or three inches long. This was the method recommended years ago before the principles of plant physiology were applied to garden practice. It is now generally recognised that the bark will never grow over such a stub, and that the end always remains exposed, and affords a possible point of entry for destructive fungi. As a rule the stub will die back, though this danger may be avoided by tarring it periodically. The current of water and food passes up and down the main stem, and the stub is side-tracked. Now, the periodic tarring would not be necessary if the bark would grow over the cut surface, and the modern pruner obtains this desired effect by cutting off the branch as close to the main stem as possible. The cut should be made parallel to the main stem and close to it ; it should not be made perpendicular to the branch cut off.

According to the old idea the cut should be made so as not to injure the bulge at the base of the branch; the modern pruner cuts right through the bulge, and endeavours to leave the stem as smooth as possible, *i.e.*, without any projecting remains of the branch. He certainly makes a bigger wound, but as the bark has only to grow on in a straight line, it heals over completely in a comparatively short time.

Pruning off large branches should never be done by a single operation. If they are sawn off close to the stem the branch falls when partly cut through, and usually tears off part of the stem. The first cut should be made about a foot away from the stem, on the under side of the branch, and continued about half-way through it. A second cut should then be made two or three inches further away from the stem, on the upper surface, and this should be continued until the branch is severed. Finally the stub should be sawn off flush with the stem. *Bailey's Pruning Book* should be on the shelves of all planters who have to deal with trees: it is the only book which treats the subject from fundamental principles. Its special parts deal, of course, with American orchard plants, and are not so much required here, but the general parts will well repay careful study.

It will be necessary to have two or three coolies on a rubber estate trained to remove dead branches and prune where it is considered necessary; they should be taught the difference between tree pruning and chopping firewood. Such a course was formerly adopted on several estates, but as diseases were not then serious it was subsequently abandoned. At the present time there is quite sufficient work, in most cases, to keep a sanitary gang in almost constant employment.

Forking.

While forking the soil of a plantation, whether in the course of an application of manures or as an independent method of cultivation, is beneficial to plant growth, there is a considerable difference of opinion as to what actually occurs when a *Hevea* plantation is forked. Many of the roots are broken in the operation, and undoubtedly in fields where the roots are matted a very large number are damaged. But while some state that this is an advantage, because it causes the tree to put out new feeding rootlets, others declare that the broken roots afford an entrance for root diseases and white ants. How far the first of these claims is justified remains to be proved, but it would seem a sound principle to require that as little damage as possible should be inflicted. With regard to root diseases, the only fungus likely to attack the broken root, as far as is known at present, is *Botryodiplodia theobromæ*, and there is as yet no recorded instance in which it has done so.

The question whether termites (white ants) will enter the tree *viâ* the broken roots must no doubt be answered differently in different countries, but as far as Ceylon is concerned the danger is small. Much misconception exists on this subject, due in great measure to lack of knowledge concerning termites and their habits. Ceylon has for a long time been fairly easily accessible, and therefore its natural history early attracted the attention of scientific travellers. Consequently it is found that nearly all authorities on termites have dealt, in part at least, with Ceylon species, so that it is now known fairly accurately what species exist in the island. Furthermore, during the last few years the habits of various Ceylon species have been investigated by many workers on the spot, with the result that it may now be claimed that more is known about the termites of Ceylon than about those of any other tropical country. But none of the collectors of the last sixty years has ever found *Termes gestroi* in Ceylon; therefore, seeing that so much has been done, it would seem a fair deduction that it does not occur in the island. The majority of Ceylon termites feed upon dead wood, and especially upon wood which is permeated with fungus mycelium; there is therefore nothing to be gained by a campaign against them, and in many cases they do more good than harm. Of course, if a tea bush is being gradually buried by a termite hill, the hill should be levelled, and termites are out of place on a lawn or in a bungalow, but in general a campaign against termites on the ground that they damage living plants is not worth the time and expense in Ceylon. There is one known exception to the general rule. *Calotermes militaris* attacks living tea bushes, and eats out the centre of the stem. But it is not of the slightest use to level every termite hill and destroy its inhabitants in the expectation of getting rid of *Calotermes militaris*, because the latter does not construct a nest in the soil, but lives entirely within the stem of the tea bush.

In Malaya the conditions are different. There *Termes gestroi* is found, and, as was recorded by Haviland, who first discovered it, it attacks living trees. Unfortunately, it has extended its ravages to the introduced *Hevea*, and hence it is necessary to take steps for its eradication. But here again a campaign against every termite on a rubber estate is an extravagant and unnecessary proceeding, because most of them are not *Termes gestroi*, but some other harmless species. However, as matters stand at present, any other selective method which would involve a recognition of the various species of termites is hardly possible, because the study of termites is a somewhat neglected branch of entomology, and there are not more than half a dozen men in the world at present who can identify the different species.

The best apparatus for destroying termites is the Universal Ant Exterminator, which injects a mixture of sulphur dioxide and arsenic into the nest. This apparatus was introduced into Ceylon several years ago, and has been found quite effective.

The Protection of Wounds.

Wounds made by pruning off large branches, or by lopping trees to get rid of 'pink disease,' or by excising extensive areas of cortex attacked by canker, must be protected in some way or other to prevent the entrance of fungi into the tree through the exposed wood. About seven years ago, on a Ceylon plantation, a number of fourteen-year-old trees which had forked near the base had one of the stems removed. The cut surface was not protected in any way; and in course of time it was attacked by fungi, which gradually penetrated into and hollowed out the remaining stem to such an extent that the trees break off in a moderate breeze.

Coal tar is the best substance to employ for covering wounds in cases where it is immaterial whether the wound ever heals over completely or how long it takes to do so. From a mycological point of view Stockholm tar is too evanescent. The latter has been universally recommended for tea, but there do not appear to have been any comparative experiments on the subject. Either will kill the living cortex if applied to it, and in that respect Stockholm tar is liable to cause most damage, because it is more fluid and therefore more likely to run.

Stockholm tar is a poor protection against fungi, and in one case, in *Hevea*, fungi grew on the cut surface three weeks after its application. Modern practice favours coal tar. W. J. Bean, of Kew, writing on pruning in the *Gardeners' Chronicle*, April 21st, 1906, stated:—'The virtues of ordinary coal tar—not Stockholm tar—as a dressing for cut surfaces are not generally known. All the raw places left by removing branches or stumps of branches should be immediately covered with this antiseptic substance, and the coating should be renewed as often as is necessary till the wound is covered with new bark. The best armour that a tree can have to protect it against fungoid enemies is that with which Nature has provided it, viz., its bark. But when accident has produced a flaw in the armour the most efficient substitute is coal tar.' Bailey, in his *Pruning Book*, describes a series of experiments with different substances, in which the wound covered with coal tar healed quickest; but he points out—what is generally overlooked—that rapidity of healing is governed more by the position of the wound than by the preservative used; he expresses a preference for white lead paint, and many orchardists agree with him on that point. Of course, when tar is used care must be taken that the coolie does not apply so much that it runs over all the surrounding healthy bark, and to avoid this it is better to apply it cold.

In the case of wounds on tapped surfaces, where the amount of wood exposed is usually small, and the chief object is to secure an even renewal as rapidly as possible, nothing is better than the usual mixture of cow-dung and clay. In such a situation tar should be avoided, unless the wound is so large or the tree so old that a complete recovery cannot be expected. Two parts of clay to one part of cow-dung forms a suitable mixture, and if some hair is mixed with it, it will adhere better. The addition of a few drops of carbolic acid might serve to keep off beetles, but this appears to be unnecessary when the wound is merely the result of tapping operations. When diseased bark is cut out and the wound plastered over with cow-dung and clay, the wood is frequently attacked by beetles, which penetrate through the mixture, but this does not seem to occur when only sound wood was exposed. In the former case the wood and its contents have undergone partial decomposition, and apparently attract boring beetles.

There is room for further experiment on the subject of protection for wounds in the tropics. The sediment from Bordeaux mixture has been found effective in temperate countries, and is more promising, for tropical use, than many other substances; this is obtained by allowing Bordeaux mixture to stand for some time until a bluish white, creamy precipitate settles down; the latter is painted over the surface to a thickness of one-eighth of an inch if possible. It is said not only to act as an antiseptic covering to the wound, but also to check evaporation from the exposed wood, so that the formation of the new wood round the margin of the wound begins under favourable conditions.

Resin oil has been used for protecting wounds in the West Indies, and is strongly recommended, but it is not yet obtainable in the East. As a rule, all grafting waxes, wax mixtures, or oil mixtures employed in temperate climates are too fluid in the tropics, and consequently they soak into surrounding healthy bark and kill it; in extreme cases they may soak so far into young trees that the transpiration current is stopped and the trees die. As a result of experiments in Germany, the following was recommended as a cheap protective for large wounds :—500 grams melted white resin, 500 grams wood tar, 125 grams printer's varnish (linseed oil varnish), and 60 grams spirit.

The various brands of carbolineum are unsuited for use on *Hevea*. These were first introduced as insecticides, but it has been found that when used at full strength they injure living trees, and when diluted they do not kill the insects. They have been employed, in temperate countries, as a winter wash, when the trees were dormant, but they kill the developing buds, and, if used at full strength, the bark also. Aderhold reports that carbolineum is not as effective as coal tar for protecting wounds. A similar product, under the name of Smearoleum, was responsible for the death of a large number of young *Hevea* in 1905. It was painted round the base of the stems in the expectation that it would keep off porcupines, with the result that it soaked right through the cortex into the wood and killed the trees.

All patent insecticides, fungicides, or protectives should be tested on one or two trees before being applied on a large scale. Attention to this point will prevent much disappointment and loss, since many of them are distinctly injurious and will undoubtedly kill such a soft-barked tree as *Hevea*.

In very many cases these patent liquids are merely waste products from coal tar, with nothing to recommend them except their smell. In case of doubt, planters should consult their agricultural department; the various technical journals contain numerous accounts of analyses and experiments with most patent remedies, and as many of these journals are published in Germany their criticism is freer than is permissible in England.

The Internal Application of Fungicides.

The treatment of fungus diseases by the injection of fungicides into the plant has been the subject of experiment for the last thirty years, without any success which would justify practical application. The idea is periodically revived; and whenever public interest is aroused in any disease, suggestions pour in that the plants should be watered with copper sulphate, or carbolic acid, or some other substance which will render them immune to disease. The following brief summary of recent work on this subject will serve to show the present state of affairs.

In 1903, Mokrschetzki published a preliminary paper in the *Zeitschrift für Pflanzenkrankheiten*, entitled 'The Internal Therapeutics of Plants.' His method consisted of placing a powder in a hole in the stem of a tree, or forcing a solution into the stem under slight pressure by a specially constructed apparatus. In one instance, apple trees affected by chlorosis were rendered green in ten days by placing 12 grams of dry iron sulphate in a hole in the stem. Similar apparatus had previously been described by Schewyrjov in 1894, and had been employed in cases of chlorosis in the Crimea in 1895–6. Mokrschetzki published a further account of his experiments in 1905. He had modified his method by placing nutrient salts as well as iron sulphate in the hole, and claimed to have obtained good results in cases of chlorosis of apple trees and gummosis of apricots, plums, &c., but experiments with fungus diseases of apple and poplar trees were not successful. Apparently he did not continue his experiments further.

There are several kinds of chlorosis; and the application of ferrous sulphate has long been recommended as a treatment for one of them. But the cause of chlorosis is unknown. Similarly the cause of gummosis has not been discovered, or rather numerous causes have been discovered, but pathologists in general refuse to admit them. It is highly probable that many cases of chlorosis and gummosis are due to physiological causes, not to fungi. Consequently we have the result that Mokrschetzki's treatment has been successful in diseases of unknown origin (probably physiological), but has failed in the case of fungus diseases.

In 1904, Massee published an account of a successful experiment in which cucumber and tomato plants, watered with a dilute solution of copper sulphate, proved immune to certain common leaf diseases. The method was adopted on a large scale in 1905, but resulted in complete failure.

More recently, attempts have been made to treat diseased apple trees by injecting copper sulphate into them. 'Copper sulphate solutions were injected through roots and through holes in the trunks of the trees, and uniformly resulted in the browning of the leaves. The time required to give evidence of the injury varied with the strength of the solution and the rate of transpiration, but it is usually short, varying from twenty-five minutes to a few hours.'

Rather more promising results have been secured by Potter, who obtained, from diseased oranges and turnips, a liquid which, when injected round the edge of diseased spots on oranges and turnips similarly affected, stopped the progress of the rot. But these experiments were conducted with stored specimens, and the liquid killed the tissues into which it was injected; the method is therefore not applicable to living plants, though it may pave the way for further discoveries.

It will be evident from the above that at the present time there is nothing to warrant the recommendation of injection methods in the treatment of plant diseases.

CHAPTER IX.

Leaf Diseases.

On Eastern plantations no leaf disease has yet made its appearance on old trees. What leaf diseases there are are confined to seedlings in the nurseries, and even there they have not caused much damage.

It is quite a mistake to suppose that because *Hevea* sheds its leaves annually it is less liable to be attacked by leaf diseases than tea, cacao, or coffee, which are evergreen, The fungi which attack the leaves of deciduous trees in many cases pass through two stages; in the first stage they produce spores which immediately convey the disease to other leaves, while the second stage is passed in the dead leaf, where different, more-resistant, spores are produced which carry the fungus over the time when the trees are leafless. Thus the periodic defoliation does not involve the destruction of the fungus; and, in actual fact the serious leaf diseases of deciduous trees are more numerous than those of evergreen trees. In the case of *Hevea* it must be added that the leafless phase is of such short duration that even the first type of spore would survive it; the tree is leafless only for two or three weeks. Moreover, as the trees do not all pass through the 'wintering stage' at the same time, there are always some in leaf, and capable of transmitting any disease to the new foliage of the others.

Helminthosporium heveæ, Petch.

This fungus attacks the leaves of nursery plants, generally when the latter are three or four feet high. It causes minute purple spots which subsequently increase in size and become circular, white, and semi-transparent, surrounded by a narrow purple-brown border. As a rule these spots do not exceed 5 mm. in diameter, but they may occur in large numbers on a single leaf. The spores are produced on short hyphæ which project from the dead tissues on either side of the leaf; they are comparatively large and may be seen with a simple lens, as long, narrow, brown, and shining objects, lying upon the white spots.

The fungus has not been found on old trees. The young plants are not defoliated by it, and as a rule it has caused so little injury that no treatment has been considered necessary. If required, the plants should be sprayed with Bordeaux mixture, applied by means of a 'mist' sprayer. The disease has been found in Ceylon, South India, and Malaya.

The following is the technical description of the fungus :—*Helminthosporium heveæ*, Petch.—Spots circular 1–5 mm. diameter, surrounded by a brown line; conidiophores scattered, simple, olivaceous, 80–200 μ long; conidia cymbiform, 8–11—septate, brown, appearing dark brown and shining by reflected light, 100–200 × 15–18 μ.

References.

Tropical Agriculturist, June 1905; Sept. 1905.
Annals of Peradeniya, vol. iii., pt. 1, March 1906.
Report of the Government Mycologist for 1905.
'Die Pilze von *Hevea brasiliensis*' (*Zeitschrift für Pflanzenkrankheiten*, vol. xviii. (1908), pp. 81–92).

A Surinam Leaf Disease.

In 1908 young *Hevea* plants in the nursery of the Botanic Gardens, Surinam, were attacked by a leaf disease. The plants had been raised from seed which had been imported from Ceylon, as well as from seed grown in Surinam. The diseased leaves acquired irregular patches, dry and brown in the centre, surrounded by concentric yellowish-green zones. On the under surface these patches were covered with a film of fungus hyphæ, readily seen with the naked eye, radiating more or less from the centre; thus the fungus was chiefly external. No fructification was found, and therefore no identification of the fungus was possible.

It was shown by infection experiments that young leaves only were attacked. To all appearances the disease was extremely infectious, yet little injury was done, since the plants soon developed new leaves. Spraying with Bordeaux mixture was attempted, but with little success, because the fungus was on the under surface of the leaf. Collecting the diseased leaves was found to require too much labour, and was abandoned. 'Though the disease is a very infectious one, trees in favourable circumstances are probably little susceptible to it. The fast spreading in the above-mentioned case must be ascribed to various accidental circumstances, especially to too close planting' (Mevr. A. E. van Hall-de Jonge, Bull. No. 24, Dept. van den Landbouw, Suriname).

Specimens of a similar disease have been forwarded to Ceylon from Bolivia. Spraying with Bordeaux mixture should be attempted in such cases, and with a mist spray it should be possible to reach the under surface of the leaf. In any case, the Bordeaux mixture will in some degree prevent the spread of the disease by protecting the unaffected leaves.

Glœosporium heveæ, Petch.

This fungus has been found only once, in Ceylon. It occurred in 1905, on the leaves of young plants about a foot high, in the nursery. The leaves turned first yellow-green, then yellow, and fell off. The spores are produced in pale brown masses on either side of the leaf. This disease differed from the other recorded leaf diseases in that there was a general death of the whole leaf, not of isolated patches. A diminution of the shade made the conditions less favourable for the development of the fungus, and the new leaves were not attacked.

Glœosporium heveæ, Petch. — Pustules light brown, scattered, irregular, flattened, erumpent, surrounded by the torn epidermis, 0·1–0·25 mm. diameter, on both sides of the leaf; spores extruded in a pale brown mass, oblong with rounded ends, hyaline, continous, $12–17 \times 2·5–5\ \mu$; basidia, $20–34 \times 2\ \mu$.

References.

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Brazilian Leaf Fungi.

Hennings has recorded three leaf fungi on *Hevea brasiliensis* in Brazil. Two of them, *Dothidella Ulei*, P. Henn., and *Aposphæria heveæ*, P. Henn., were said to be very injurious, but no account of the diseases caused by them has been published. They were found on the leaves of seedlings, and the third species, *Phyllachora Huberi*, P. Henn., occurred with them.

Reference.

Hennings, P.—'Ueber die auf *Hevea* arten bisher beobachteten parasitischen Pilze' (*Notizb. d. k. Bot. Garten und Museum zu Berlin*, No. 34, Bd. IV. (1904), pp. 133–8).

A False Alarm.

In 1904 the discovery of a leaf fungus, 'probably one of the *Uredineæ*,' on *Hevea brasiliensis* in the East, was announced; it was said to have five-septate teleutospores. Nothing further has been heard of this disease, and it is most probable that it was really either *Helminthosporium heveæ* or *Pestalozzia palmarum*, neither of which is at all serious. It is fortunate that the full significance of the announcement was not understood by the planter. The occurrence of one of the *Uredineæ* on *Hevea* would in all probability have reduced its cultivation to a level with that of coffee, for it is one of the *Uredineæ*, *Hemileia vastatrix*, which causes the well-known devastating coffee leaf disease. Had that fact been grasped, the announcement would have been likely to put an end to *Hevea* planting altogether. Fortunately, *Hevea brasiliensis* remains free from any fungus belonging to the order *Uredineæ*.

Miscellanea.

It would be possible to draw up a long list of fungi found on the leaves of *Hevea*, but the majority of them have occurred under circumstances which leave little doubt that they are for the most part only saprophytic. A list of some of these will be found in a later chapter. The leaves of nursery plants provide a large number of such fungi. The young plants are grown under the shade of cadjans, &c., and when the shade is removed the planter is frequently alarmed to find that the plants are apparently suffering from a severe attack of leaf disease. The leaves bear large, white, semi-transparent patches, of varying shape and extent, dotted with black fructifications of various fungi. But practically in all cases these fungi are not the cause of the dead patches, but have only developed on the leaves after they have begun to die. The patches are actually due to the action of sunlight on the young leaves. The cadjan shade, as it ages and decays, allows the sunlight to penetrate through the cracks and to fall on the previously shaded, tender leaves with the result that they are locally killed. In some instances drops of rain water on the cadjan screens focus the sun's rays on the young *Hevea* leaves and produce a distinct burn.

Variations in the size or shape of the leaves of *Hevea* have frequently been reported, and have often given rise to the idea that a tree was attacked by some disease. There is quite a large number of small-leaved trees, and on some of these none of the leaves is more than three or four inches in length, instead of the normal eight or ten inches. On the other hand, long, narrow leaves, more than a foot in length and only about an inch in breadth, have been produced by some *Hevea* trees in Java. There is no sign of any disease on these trees, and no reason to suppose that these phenomena are due to disease; such variation is only to be expected when a large number of plants are grown under new conditions.

Climatic Leaf Fall in *Hevea*.

In August 1909 an extensive defoliation of the older *Hevea* trees occurred in low-country districts in Ceylon. In most cases the trees were only partly defoliated, the leaves falling especially from the outer branches and leaving bare shoots all over the outside of the head. In some instances all the leaves were shed, while in one case, where the trees were exposed to the south-west wind, they became bare on the south-west side only. In some respects, this defoliation resembles 'dieback,' but it is distinguished from that by the fact that the bare shoots occur all over the crown, and not only at the apex. A large number of leaves were examined, but in no case was any fungus found on them ; the leaf fall was normal, in so far that it occurred in exactly the same way as the usual leaf fall when the trees 'winter.' Dead trees were shown me in several cases, but in every one of these death was due to root disease, and not to whatever was causing the fall of leaf. Root disease was carefully looked for in all cases, but none could be found on any but the few dead trees. The general leaf fall could not be attributed to root disease, and the occurrence of a few deaths from the latter cause is only to be expected on large estates. Dead branches, usually small branches, occur on the defoliated trees, but the branches from which leaves were seen to fall were not diseased in any way. Many samples of these dead branches have been sent in for examination ; in most cases they had been dead for a long time, and bore only saprophytic fungi. Such branches are usually found in the interior of the head, and they occur quite naturally in such a position. They are killed either by shade or because they are robbed of their food supply by adjacent stronger branches. When the tree drops its leaves, these branches are picked out as samples of disease because they are dead, but they might have been gathered probably twelve months previously, and have no connection with the abnormal leaf fall. However, some branches did die after the fall of leaf in the worst cases.

Our records show that the last occurrence of this leaf fall on an extensive scale was in 1903. There was some in 1907, but only on a few estates. The absence of any fungus, and a comparison of the weather conditions in 1903 and 1907 lead to the conclusion that this leaf fall is not due to any disease, but is brought about by an abnormal rainfall. Our present knowledge of *Hevea* does not enable us to make any definite statements with respect to its behaviour on different soils and under varying conditions, and the following considerations derived from experience gained with other trees must therefore be regarded as suggestions only.

Trees are often injured in poorly drained soils during a wet period. The presence of air in the soil is essential for the growth and vitality of the roots. If the roots are deprived of oxygen they cease to perform their natural functions, and this stoppage would produce, first of all, a fall of leaf, and, subsequently, if prolonged far enough, the death of the tree. The amount of air in the soil depends on the size of the soil grains and the amount of water present; if there is too much water the circulation of air is prevented, and the roots are affected. It is evident that close-grained soils will suffer most in this respect. Further, this water-logging is brought about more rapidly where a substratum of rock or a 'hard pan' lies near the surface, or where the water table is usually near the surface. 'Hard-pan' will cause a 'dieback' of the top branches without any assistance from this water-logging after the trees have attained a certain age in some cases, but that does not apply to the present phenomenon in *Hevea*. But there is some evidence that the leaf fall in *Hevea* is associated with soil and weather conditions combined, since it is not always universal over a given field, and in some instances it is confined to two or three trees in the middle of a large group of healthy trees. This last fact counts against the view that it is due to climatic conditions only.

Of course there are well-known instances in which *Hevea* is planted on swampy land, and is subject to periodic inundations during the monsoon rains. One such plantation was under water nearly every other week during the south-west monsoon of 1909, but no leaf fall occurred. But this does not affect the question. Trees in such situations develop an enormous number of feeding roots at the surface, so much so that one is at times walking over a spongy covering of fine white roots: they have adapted themselves to their situation, and are not injured by the swampy soil. Injury only arises when trees are subjected to conditions to which they have not been accustomed, where a normally dry soil remains water-logged for some time. An excellent example of the effect of changed conditions must have been experienced by any one who has bought pot plants from the average florist; the plants, probably brought straight out of the greenhouse, are taken into an ordinary room, with the result that they frequently lose all their leaves. The plant is affected by the change of environment.

In illustration of the weather conditions during the south-west monsoon from 1903 to 1909, the rainfall records of three estates in the same district, and a fourth in another district where the leaf fall was worst in 1909, are given below; the figures are in inches:—

ESTATE A.

	1903.	1904.	1905.	1906.	1907.	1908.	1909.
May ...	26·00	24·64	21·37	15·43	19·75	14·20	20·77
June ...	14·84	18·62	16·47	20·08	13·39	11·20	15·30
July ...	12·52	24·13	7·48	6·56	11·64	9·66	17·61
August ...	13·08	0·43	5·14	8·13	17·59	3·79	23·89

ESTATE B.

	1903.	1904.	1905.	1906.	1907.	1908.	1909.
May ...	17·14	23·11	25·64	12·75	16·38	18·42	16·51
June ...	10·00	19·36	17·43	11·18	17·93	8·46	18·79
July ..	13·73	13·28	4·19	8·23	5·36	6·16	12·68
August ...	25·54	1·09	4·44	8·85	16·09	2·73	26·27

ESTATE C.

	1903.	1904.	1905.	1906.	1907.	1908.	1909.
May ...	26·23	25·85	21·64	13·31	17·18	14·18	18·55
June ...	16·20	17·70	14·18	12·37	15·78	10·32	15·13
July ...	12·71	18·88	5·23	8·62	12·88	8·80	14·26
August ...	13·79	1·32	7·35	11·32	19·95	5·15	22·59

ESTATE D.

	1903.	1904.	1905.	1906.	1907.	1908.	1909.
March ...	7·87	6·99	6·00	3·25	13·48	13·77	21·33
April ...	16·72	14·73	22·92	7·65	25·79	9·73	20·24
May ...	23·15	29·03	29·08	13·55	14·54	18·75	16·83
June ...	31·90	32·85	32·42	20·49	21·79	24·06	24·30
July ...	22·60	29·91	19·37	23·30	18·07	9·03	27·47
August ...	11·71	4·67	13·54	15·38	20·84	12·79	28·81

In considering these figures it must be remembered that a continuous rainfall is more injurious than a heavier fall in a shorter time. Leaf fall apparently occurs when the heavy rains of June and July are continued through August.

When the rains cease the trees begin to produce new leaves. One tree which was practically leafless in September was in full leaf a month afterwards ; this was in a district where the heavy rains continued throughout September. But complete recovery is not usually effected until November. As far as I have seen, all the trees recover during the drier weather, and, beyond the check in growth, no injury is caused. Defoliation must, of course, diminish the amount of food manufactured by the tree, and therefore cause a diminution, or cessation, of growth.

Pruning of dead branches should not be attempted when trees have been defoliated by continued heavy rains, except in obvious cases of 'pink disease' or 'dieback.' When the branches are leafless the coolie fails to distinguish between the living and the dead, and cuts off many which are quite sound. It is better to wait until the fine weather, when the sound branches will produce leaves again.

CHAPTER X.

Root Diseases.

When the roots of a *Hevea* are attacked by a fungus they are no longer able to perform their proper functions, and thus the supply of water to the stem and leaves is cut off. The symptoms of a root disease are therefore purely secondary, for the fungus has rarely advanced above ground by the time the tree is dead, and the upper parts die from lack of water, not from an attack of the fungus on those parts. It follows that the symptoms of all root diseases are practically the same, and are such as might be expected to follow if the trees were subjected to a prolonged drought. If the tree is a small one the leaves suddenly turn brown and dry up, and it dies with all its leaves attached ; at the same time the cortex dries, and no latex exudes if it is cut. Large trees may die more gradually, the green shoots dying back first of all, and frequently the leaves fall off, but the final stages are usually rapid, as in the case of young trees. The difference depends upon the extent to which the roots have developed ; death is always sudden in young trees which have not acquired large lateral roots, but where the latter are present the tap-root may be completely destroyed without any apparent ill-effect to the tree, and in such cases there is often no indication of disease until the tree is blown over. When trees are blown down, root disease should always be suspected.

Cases of root disease are often mistaken for 'dieback.' In the latter disease, unless very far advanced, the lower part of the stem is quite healthy and yields latex when cut ; and there can be no confusion in the earlier stages, for in 'dieback' the lower branches are still green and in full foliage when the upper branches are dead.

There is little hope of saving a tree which is attacked by root disease. If its condition is discovered when only one lateral root is affected, then the latter can be cut off ; but this is rarely possible, both because the disease is not

A.—*Fomes semitostus.* × $\frac{1}{4}$.

B.—*Hymenochæte noxia.*

Plate II.—ROOT DISEASE OF *HEVEA*.

detected at such an early stage and because the tap root is generally involved. All measures must therefore be directed to getting rid of the diseased trees and the source of infection and preventing the spread of the fungus to others. A root disease is not so much to be feared as a leaf disease. True, the affected trees must die; but the disease is confined to certain centres, and the planter knows exactly where he stands. In general, he has not to face a possible rapid dissemination of the disease by means of spores, which may infect all his trees at any moment; he is dealing with a localised mycelium, and if the proper treatment is carefully carried out he can eradicate it.

Three root diseases of *Hevea* are known up to the present. The following details will serve to distinguish between them, after the dead tree has been dug up. When the dead root is covered with white threads, which in some places may spread out in a white superficial film, and in others may be united into thick, white or yellowish cords, the tree is attacked by *Fomes semitostus.* If the root is encrusted with a mass of sand and small stones, cemented together by brown or black mycelium—as though it had been coated with glue and thrust into the soil—the cause of the disease is *Hymenochæte noxia.* Finally, when the root is quite clean externally, although decayed, and dark red or black strands are found between the wood and the cortex of the root, it is attacked by *Sphærostilbe repens.*

A diagnosis, based upon the particulars given above, is only likely to be wrong in the case of the first of these diseases. Most of the larger fungi have white mycelium, and therefore it is, as a rule, impossible to say to what species of fungus any white mycelium belongs, if the fructification has not developed. Any decayed wood will be found to be permeated with white mycelium, and the soil of a newly-opened plantation is usually full of pieces of wood, leaves, branches, &c., bound together by white fungus threads. Such threads, in general, belong to *saprophytic fungi*, *i.e.*, species which can only live on dead tissues and cannot injure living plants; and they may cover a *Hevea* root without causing any decay. But when white fungus threads are found on a *dead Hevea* root, and extending upwards over the healthy bark, they may, in the present state of our knowledge, be safely attributed to *Fomes semitostus.* Of course, if some other fungus which possessed white mycelium was found to be parasitic on *Hevea*, it would be necessary to find some other character by which to distinguish between them.

Fomes semitostus, Berk.

This disease was first discovered by Ridley at Singapore in 1904, the fungus being identified by Massee. It appeared on the roots of trees which had been injured by fire, and in this respect its first recorded occurrence differs from most of the subsequent cases. In that instance it was apparently a wound parasite, *i.e.*, it attacked trees which had been previously injured, but it has since been abundantly demonstrated that this fungus does not require the assistance of any previous injury, but can attack quite healthy trees.

In Ceylon it was first discovered in 1905. The absence of any recorded occurrence prior to that is doubtless due to the fact that much of the earlier Ceylon *Hevea* was planted among tea or cacao, and that some of the earliest plantations were established on land which was cleared of almost all the jungle stumps before planting. The first cases were sent in as examples of the damage done by white ants, as the tap-roots had been eaten away by those insects; but a microscopic examination showed the presence of a fungus mycelium in and on the dead roots, and the fructification of the fungus was subsequently developed from them. The trees had shown no sign of disease until several were blown over, when it was found that their tap-roots had decayed and had been eaten away. These trees were two years old (from planting), and had developed rather strong lateral roots; these had been quite sufficient to provide an adequate supply of water, but were unable to support the trees in a strong wind.

Since 1905 this disease has been found to be fairly common in one district in Ceylon, and very common in Malaya. Specimens have also been received from South India; while others from the Gold Coast were identical, at least as far as the mycelium was concerned, and would have been assigned to *Fomes semitostus* without fear of error had they been Ceylon specimens. A similar disease occurs in Java (*champignon blanc des racines*), but the fructification of the fungus has not yet been discovered there. There is little doubt but that *Fomes semitostus* will be found to cause root disease of *Hevea* throughout the tropics.

There has been some discussion as to whether, when trees blow over owing to loss of the tap-root, their loss is to be attributed to white ants or to root disease. It has been stated that in some cases neither of these can be said to have killed the tree, since it is not dead when it falls over; this last fact is doubtless true, but the cause of the injury is, primarily, the root disease. If the tree possesses well-developed lateral roots, it is rare that any sign of the disease will be evident above ground before it blows over. The root disease kills the root, and the white ants eat away the diseased wood; in such cases the white ants play a secondary part, and are distinctly beneficial, since they prevent the spread of the mycelium and the development of the fructifications by devouring the diseased tissue. White ants cannot, however, be relied upon to exterminate the fungus altogether, because their operations do not, as a rule, extend over the whole of the diseased wood; they are always a little distance behind the advancing edge of the fungus. As far as Ceylon is concerned, white ants have not yet been known to bring about the death of a *Hevea*. The case is doubtless otherwise in countries where *Termes gestroi* occurs, but we are still without information which will enable the planter to differentiate between trees attacked by *Termes gestroi* and trees attacked by root disease followed by termites, except by identification of the insect, which for the majority of planters must be impracticable.

The diseased root is covered with a white mycelium (Plate 2A), in some places forming a continuous felted sheet, and in others aggregated into stout white or yellowish strands which run irregularly over its surface. These strands may be as much as a quarter of an inch broad, dividing into smaller branches higher up the root; they are the more characteristic sign of *Fomes semitostus*, and are always present, whereas the white felt may be wanting. If the strands of mycelium have reached the collar they usually divide there into much finer threads, which may be detected by careful examination on the rough bark at the base of the stem. These, however, do not appear above ground until the root is almost entirely destroyed, and they would escape the notice of any one who was not carefully examining every tree. The depth of the tap-root which is covered by the mycelium may be as much as thirty inches. This external mycelium gives rise to threads which penetrate into the tissues of the root. The whole of the wood and cortex is permeated with fine, white fungus threads, which ultimately render them soft and friable: this internal mycelium spreads upwards chiefly in the outer tissues first, and therefore at the collar it is found mainly in the cortex. The diseased wood is not noticeably discoloured, and although the cortex is decayed and does not yield latex when cut, it does not appear, to the naked eye, much different from the usual cortex, except for its covering of mycelium. If a diseased root is kept in a damp chamber it develops a white feathery growth of rather long silky hyphæ; while if it is planted in a pot, covered with a bell glass and kept moist, the fructification will be developed in a few months.

The fructification of the fungus will seldom be found on *Hevea* which has been killed by its mycelium; it can only be formed above ground, and, as a rule, the mycelium has not reached the surface by the time the tree is dead or blown over. Of course, if the dead tree is left standing for some months the fructification will ultimately develop at its base, unless prevented by white ants; and if a fallen tree, killed by *Fomes*, is allowed to lie and decay, the fructification will similarly be developed along its whole length. But under ordinary estate conditions, where a dead tree is discovered soon after it has died, neither the stem nor the root will show any sign of a fructification. But it is practically always possible to find it in abundance on a neighbouring jungle stump, at least in young plantations.

The fructification, or sporophore, first appears as a small orange-yellow cushion. This grows out horizontally into a flat plate, more or less semi-circular in outline, attached to the stem of the tree along its hinder margin. In general this plate is about four inches long and two and a half inches wide, but in favourable situations it may measure a foot in length; it is about a centimetre thick behind, and thins out regularly towards the margin. From their shape fungi of this class are known as 'bracket fungi'; they are quite common on decaying logs and stumps, but the majority of them are harmless.

Fomes semitostus is identifiable by its colours; but the colour varies enormously according to the amount of moisture in the fungus. When fresh the sporophore is a rich red-brown on the upper surface, with a bright yellow margin, while its under surface is a bright orange. As the fungus dries, the red-brown colour of the upper surface gradually disappears, not uniformly all over, but in concentric zones, so that it becomes banded with broad alternate zones of red-brown and pale yellow-brown. Finally, when quite dry, the upper surface is pale yellow brown marked with concentric darker lines, while the under surface is reddish brown.

The upper surface is not smooth, but bears numerous concentric grooves parallel to the outer edge; it is these grooves which retain the red-brown colour when the fungus is dry. Fine radiating striæ run at right angles to the edge, and give the surface a silky appearance. The lower surface is studded with minute holes, which are the openings of the tubes in which the spores are produced; these holes are very minute, and scarcely distinguishable without a lens.

The substance of the fructification is somewhat woody, but it can easily be broken between the fingers. The interior consists of two layers, differing from one another in colour and structure. The upper layer is white and fibrous, the fibres running more or less parallel with the surface, but the lower layer is red-brown and consists of closely packed tubes perpendicular to the under surface.

When the fungus is allowed to luxuriate unchecked these plates grow one above the other for a distance of two or three feet, joined behind by a continuous orange-yellow cushion; at the same time fresh plates are produced at the sides of the old ones, and these fuse together and make the edge of the sporophore more irregular. Such masses of sporophores may extend along fallen logs or the lateral roots of jungle stumps for several yards.

As a rule, this disease makes its appearance when the plantation is from one to three years old. Isolated cases have occurred in which it has attacked older trees, but where the disease has been widespread young trees have usually been concerned. This is not due to any immunity conferred by age upon the *Hevea*, but to the manner in which the disease originates. Practically in all cases the fungus first develops upon a neighbouring jungle stump; the spores of the fungus are blown on to the dead stump, and after they have germinated the resulting mycelium penetrates into the wood and gradually consumes it. After some time, when the whole stump has become permeated with the mycelium, the latter spreads out through the soil in search of other food, either along pieces of wood or independently. Many fungi can only travel *within* wood or branches, &c., but *Fomes semitostus* can advance through the soil unattached to any tissues, either living or dead. If this mycelium meets the root of a *Hevea* it immediately grows round and along it, emitting threads which penetrate into the tissues and ultimately destroy it. Thus the time when the disease is first manifested depends upon the time taken by the fungus to destroy the jungle stumps. It may also be dependent to some extent upon the time taken by the *Hevea* to develop lateral roots, since the fungus will then have a smaller distance to travel to reach them. It is not, however, necessary that the lateral roots of *Hevea* should come in contact with diseased roots, since the threads of *Fomes semitostus* can travel independently through the soil.

By the time the *Hevea* begin to die, the jungle stump from which the disease has spread is covered with the fructifications of the fungus. I have never found any difficulty in determining which stump the fungus spread from in young clearings. In one instance, where no stump was evident anywhere near, the fructifications were discovered on a log which had been buried beneath about six inches of earth and stones so that one end of it came within a foot of the root of the dead *Hevea*. In two instances, the original jungle stump had been completely devoured by white ants, but the fructifications had developed within the galleries of the termite hill.

It may be laid down as a general rule that young *Hevea* is not attacked by *Fomes semitostus* except through the agency of a jungle stump. But old *Hevea* may be attacked long after all jungle stumps have disappeared, and in such cases it is most probable that the fungus develops directly upon the trees from spores which germinate upon injured and exposed lateral roots. For a long time *Fomes semitostus* had not been recorded on old *Hevea* interplanted among tea; but one such case was found in 1909, the *Hevea* being over twelve years old and the tea over twenty-five years. In that instance there was no possibility of infection from a jungle stump. Such cases are, however, rare in comparison with the number which occur in young plantations.

Since it is scarcely possible to detect root disease before the affected tree is dead, all remedial measures must be directed towards preventing further loss. The dead trees must be removed as completely as possible, and burnt. As a rule, dead trees occur in patches, with a decaying jungle stump somewhere about the middle of the patch. This stump must be dug out and burnt. It is of little use to remove the dead trees only, and leave the stump from which the fungus spread. The apparently healthy trees round the affected area should also be examined by laying bare their tap-roots, as it is quite possible that the threads of the fungus may have already reached them; if their tap-roots are decayed the trees must be removed, but if only one of the lateral roots is attacked it should be cut off. In some cases it has been noted that when the tap-root has been destroyed and the tree is being supported by its lateral roots only, the stem becomes fluted at the base; the increase in girth is apparently greatest over the main lateral roots, so that a ridge is developed on the stem vertically above each of them.

When the extent of the affected patch has been determined, a trench about eighteen inches deep must be dug round it, as far away as possible from the centre. It will generally be found most convenient to dig the trench about midway between the rows of trees. The ground enclosed by the trench must be dug over, and all dead wood removed. The lateral roots of the jungle stump must be followed up and extracted. In many instances a trench is dug round the stump, cutting through the latteral roots, but nothing is removed, the excuse being that the expense of removal is too great. The stump then remains as a source of infection, covered with fructifications which produce myriads of spores, while the severed lateral roots, exposed in the trench, provide additional surfaces for their development. Even if the lateral roots were severed beyond the diseased region, the sporophores borne on the cut surface will liberate spores which will infect the pieces beyond the trench, and the latter will then spread the disease further, just as if the trench had never been dug. It is impossible to get rid of *Fomes semitostus* if the stumps which bear the fungus are not removed; neglect of that operation is the chief source of failure.

The affected ground should be dug over to a depth of about two feet, beginning at the centre. If lime is obtainable it should be forked in at the same time; this will assist in destroying the fungus, since most fungi do not flourish in an alkaline medium. The patch should be kept clear of weeds, and be dug over at intervals of a month or two, three or four times. It is scarcely worth while to replant with *Hevea* in less than twelve months.

On many estates, all dead stumps have been extracted and all dead timber removed. If *Fomes* is general over the whole estate such a course is decidedly necessary ; it was followed on one estate in Ceylon in 1906, where 700 trees blew over in a single night on a field of about eighty acres. But in the majority of cases, such procedure, though it will certainly ensure freedom from the attack of *Fomes*, if carried out early enough, must be considered somewhat extravagant. *Fomes semitostus* does not grow on any kind of stump ; it attacks stumps of certain species, and it should be sufficient to mark and remove those which are known to harbour it. In Ceylon, it is generally found on jak stumps (*Artocarpus integrifolia*), but it has also occurred on Bombax (*Bombax malabaricum*). Gallagher records that he discovered the fructification in one instance on a serdang stump (*Livistonia cochin-chinensis*) in a *Hevea* plantation which had apparently never been attacked by disease ; but it is probable that this record is incorrect. In another account he states that he has found the disease spreading from meranti (*Shorea sp.*), and merbau (*Afzelia palembanica*) stumps. Probably more information is required before any selective method of stump extraction can be adopted.

A large number of jak trees was felled on the Experiment Station at Gangaruwa during 1902–04, and the stumps of these were subsequently covered with the sporophores of *Fomes semitostus*. But no root disease occurred among the tea, cacao, or dadaps (*Erythrina*), though these plants were in many cases in contact with the decaying stumps. It may be deduced that these products are not attacked by this disease. The statement that it has been found to attack *Crotalaria* in Ceylon is incorrect.

The fructification of *Fomes semitostus* is fairly easily identifiable, especially in the fresh state, and every rubber planter should make himself acquainted with it. But the published accounts of the disease show that at least one other species is being mistaken for it. When dry, this species closely resembles *Fomes semitostus ;* as in the latter, the upper surface is then pale yellow-brown, with narrow red-brown concentric lines. But it is usually smaller, is not wholly red-brown when moist, and is grooved radially as well as concentrically. The chief difference, however, is in the colour of the lower surface, which, instead of being orange-red or red brown, is a dingy livid grey. This species is *Polyporus zonalis*, Berk.; it is common on dead wood everywhere, and especially common on dead palms and bamboos. It was probably this species which occurred on *Livistonia*.

The fructification of *Fomes semitostus* is never 'hoof-shaped.' The illustration published in the Peradeniya circular on this disease has probably assisted in giving the impression that it is, but the specimen there depicted was a flat plate. It was photographed rather obliquely.

Ridley has recorded that attempts to kill the fungus by soaking the ground with Bordeaux mixture, or by digging in solid copper sulphate and lime have been unsuccessful. In these cases the fungus was within the roots of the *Hevea*, and could not be reached by any fungicide, at least if applied in reasonable quantities. It is useless to attempt such methods without removing all dead wood and stumps; fungicides could only kill the free mycelium in the soil. Ridley recommends that infected areas should be planted up with bananas, the roots of which might break up the small pieces of dead wood in the soil.

Fomes semitostus, Berk.—Perennial, woody, imbricated: pileus dimidiate, about 10 cms. long and 6 cms. broad when simple, becoming larger by confluent side growths, at first red-brown with a yellow swollen edge, then pale yellow-brown with concentric dark brown lines, smooth, feebly sulcate, faintly striate radially, slightly silky with adpressed fibrils; thickness (with two layers of pores) 1–1·5 cms.; margin thin, entire; pore surface orange, red-brown when old; pores minute, 0·06–0·12 mm. diam., rather widely separated, stratose, red-brown in section and 2·5–3·5 mm. long; flesh white, woody, with concentric lines of growth curving from the hymenium to the surface sulcæ.

The above description refers to the fungus which is now known as *Fomes semitostus* in Ceylon, South India, and Malaya, but there is considerable doubt whether it is really the same as the species to which Berkeley applied the name: it is probably *Fomes Auberianus*.

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Brown Root Disease (*Hymenochæte noxia*, Berk.).

This disease was first recorded on *Hevea* in Ceylon in 1905; it had previously been described on coffee in Java by Zimmermann, and was originally found on breadfruit trees in Samoa. It is probably the commonest root disease of *Hevea* in Ceylon, though it does not cause so much damage as *Fomes semitostus*. The latter can spread independently through the soil from a jungle stump, and may attack a number of trees on the same spot before any one of them shows signs of disease. Brown root disease, on the contrary, spreads extremely slowly, and only along the roots of the tree; it does not, therefore, infect the neighbouring trees unless their roots are in contact with those of the diseased tree, and its progress is so slow that, as a rule, the first affected tree is dead before the neighbouring trees are attacked. In general, therefore, only one tree is killed at each centre of infection, unless the dead tree is left standing for two or three years.

When the dead tree is dug up the special characters of brown root disease are immediately evident, and there can be no mistake in the diagnosis. The roots, especially the tap-root, are encrusted by a mass of sand, earth, and small stones to a thickness of three or four millimetres, and, as a rule, this crust extends up the stem for several inches (plate 2, B). This mass is cemented to the root by the mycelium of the fungus, which consists of tawny brown threads, collected here and there into small sheets of nodules. In the early stages the predominating colour is brown, and the name given to the disease then appears more or less appropriate, but as it grows older the fungus forms a black, continuous covering over the brown masses of hyphæ, and the diseased root then appears chiefly black. In all stages, however, the encrusting mass of stones and earth, intermingled with brown threads, serves to distinguish it.

If the outer coat is scraped away, the cortex of the root is found to be decayed, and usually coloured brown by the fungus. The wood is soft and friable, and usually yellow, with wedges of brown, decayed powdery tissue penetrating from the exterior towards the centre. Black lines frequently run irregularly through the wood, and it is sometimes possible to trace a network of fine brown lines if the root is split longitudinally.

Although this disease has been known in Ceylon for the last six years, and very many specimens of it have been examined during that period, the fructification of the fungus has been obtained on only a few occasions. As a rule, the dead tree is dug up and removed before any fructification has developed; and it has not been found possible to grow it in the laboratory from the diseased roots. Even when trees killed by this disease have been left standing for several years they have not produced it. It takes the form of a thin dark-brown crust, adhering to the base of the stem. In other countries it is said to completely cover the stem for a length of several inches, but its growth in Ceylon is not so luxuriant, and it only forms a small patch an inch or two in diameter. On examination with a lens this patch appears finely velvety, being covered with minute projecting bristles. These bristles, which are the characteristic feature of a *Hymenochæte*, are sub-cylindrical, blunt or pointed at the apex; they project 30–110 μ above the surface, and measure 6–10 μ in diameter.

Hymenochæte noxia is not confined to *Hevea*. In Ceylon it has been found to attack cacao, tea, dadap (*Erythrina*), *Castilloa elastica*, caravonica cotton, camphor, *Cinnamomum cassia*, *Erythroxylon coca*, and *Brunfelsia americana*. Specimens have been received from the Gold Coast on *Funtumia*. Zimmermann discovered it in Java on coffee; and Brick has recorded it from Samoa, where it attacks cacao, Castilloa, *Artocarpus incisa* (breadfruit) and *Albizzia stipulata*, as well as jungle trees. It was originally discovered in Samoa, whence it was sent to Berkeley with the information that it caused great injury to breadfruit trees; it is apparently a more serious parasite there than in Ceylon, since it is sufficiently destructive to have attracted the attention of the natives, who know it under the name 'Limumea.'

Ridley (*Agric. Bull. Straits*, July 1909), when quoting the record of *Hymenochæte noxia* in Apia, stated that he had noticed Para rubber trees killed by a species of *Hymenochæte*, which was probably *Hymenochæte noxia*. He described it as having the appearance of a thin layer of bright brown velveteen on the tree or root, and seeming to hold to itself the soil, sand, &c., making a thick coat upon the root. Gallagher (*Bull. No.* 2, *Department of Agriculture*, Federated Malay States) also records a root disease 'caused by an unidentified fungus.' He states: 'The fungus gathers up earth into a black, rough felt against the root, so firm that it cannot be washed away. When this is cut into numerous small stones will be met. The way in which the fungus binds up pebbles and earth into a mass round the tap-root is quite remarkable.' This description applies exactly to *Hymenochæte noxia*, which is, no doubt, common in Malaya, though it may hitherto have been confused with *Fomes semitostus*.

Anstead, in the *Planters' Chronicle*, vol. iv., p. 277, has recorded what is probably the same disease in Southern India. He states; 'When a dead plant is dug up, the main roots are found to be rotten, while round the collar and below it is a black charcoal-like mass cementing earth and stones to the plant.' This description fits advanced cases of *Hymenochæte noxia*, since the brown patches of mycelium on the root acquire a black outer crust when old.

The occurrence of this disease on old tea and old *Hevea* where no jungle stumps remain would seem to point in those cases to an infection by wind-borne spores. But, according to our present knowledge, a further spread by means of spores from the plant first attacked cannot happen under ordinary conditions, because the fructification, if it is formed at all, is not produced until the plant has been dead for a very long time. If the dead tree is allowed to remain the mycelium spreads along the roots to the roots of adjacent trees which are in contact with them; but this is a slow process in this case, and should not happen with ordinary care.

In one instance this disease (on tea) was definitely associated with decaying stumps. These were 'Na' (*Mesua ferrea*) stumps which were at least fourteen years old; the fungus had first attacked the stumps and then spread along the roots to the surrounding tea bushes. But in the majority of cases the fungus cannot be found on decaying jungle stumps; and it would seem that most cases must result from spore infections, except on old cacao land.

Brown root disease is the only root disease of cacao known in Ceylon. Comparatively few cacao trees are killed by it, but it develops freely whenever the cacao is cut down. Nearly all the cases of *Hevea* attacked by this disease in Ceylon come from old cacao land which has been cleared in order to plant *Hevea*, or from estates where alternate lines of cacao have been cut out for the same purpose. In such cases it may be exceedingly troublesome, not because it spreads from one *Hevea* to the next, but because each cacao stump may be an independent centre of disease. Seeing that it was known to attack both products in 1905, it is surprising that the cacao was not uprooted in such cases, instead of being cut down. There is no doubt that on many estates where cacao and *Hevea* have been interplanted the cacao will ultimately have to be removed. When that step becomes necessary, the cacao must be uprooted; if it is cut down, and the stumps are allowed to remain, root disease will certainly attack the *Hevea.*

In illustration of the rate at which the disease spreads, the following instance may be cited. *Hevea* was planted, fourteen feet apart, in a single line round the boundary of an old-established cacao estate. When the trees were eight years old one of them died, from brown root disease as was subsequently discovered. The tree was left standing and allowed to decay. Two years later the next tree in the line died and was likewise left to decay. After a further two years had elapsed the next tree in the same direction along the line failed to recover after 'wintering,' and was evidently dying; and an examination of this tree and the two old decaying stumps proved that they had all been killed by *Hymenochæte noxia.* Some of the surrounding cacao was also killed during the four years, but the path of the fungus from one *Hevea* to the next had been along the *Hevea* roots.

Dead trees should be removed, with as much of the roots as possible, and burnt. In the case of *Hevea* planted on old cacao land, any neighbouring cacao stump should be dug up at the same time. As a rule, the whole of the fungus is removed with the dead tree; apparently it does not travel independently through the soil, but only in contact with roots or dead wood. Consequently it is rarely found that a neighbouring tree dies after the first dead tree has been got rid of. But to make certain that the fungus is destroyed, it is advisable to dig over the affected spot, collect and burn any dead wood, and fork in quicklime. It should be possible to replant in the same spot within a very short time; the experiment of immediately replanting the same species in the place where the original plant was killed by this disease has been carried out, but it is, as yet, too early to be sure that the 'supply' will not be attacked.

During 1909 a *Hevea* of about twenty years old at Henaratgoda was found to be decayed on one side near the base. The decay had originated on one of the lateral roots and had spread to the main stem. This was determined to be due to *Hymenochæte noxia*, and an examination of the surroundings showed that, at some time or other, twelve trees had died in that particular spot, judging from the vacancies and the remains of *Hevea* stumps. In all probability this disease had been killing trees in that spot ever since the establishment of the plantation. The decayed root was dug up, all diseased wood cut from the trunk of the tree, and the wound tarred; and as the tree possesses other large lateral roots it is expected to survive.

Hymenochæte noxia, Berk.—Broadly effused, encrusting, firmly adnate, margin sub-indeterminate; hymenium dark brown, minutely velvety; setæ subcylindrical, blunt or pointed, 30–110 × 6–10 μ.

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SPHÆROSTILBE REPENS, B. AND BR.

This disease was first recorded in the report of the Ceylon Mycologist for 1907. Since then it has been found on four or five occasions, but in no case has it caused any widespread damage. It has not yet been reported from other countries.

In the first case recorded it killed three large trees about twenty-five years old. These stood in a patch of undrained sour soil, between a set of coolie lines and a factory, where their surface roots were constantly being damaged. This

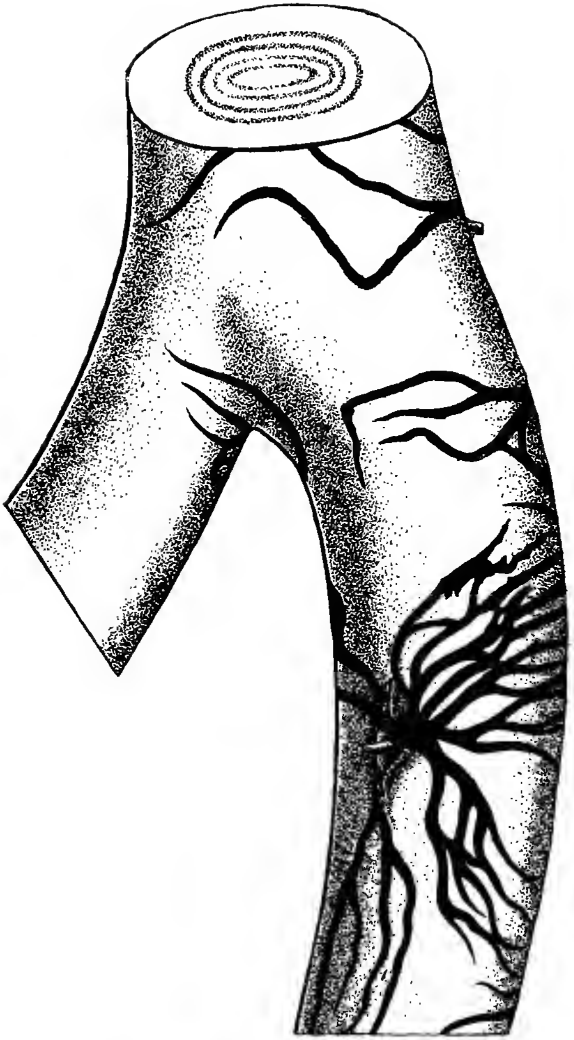

Plate III.—SPHÆROSTILBE REPENS.

Mycelium on wood of root.

area was used as a storage ground for firewood, and it is most probable that the fungus was introduced with the dead wood and entered the damaged roots. In another case a single tree about twelve years old, which stood in swampy ground at the side of a stream, was killed. In each of two other cases an old tree, again in swampy soil, was killed. Judging from these cases the disease would appear to be most frequent in swampy or sour soil, but in another instance it killed young trees, about two years old, in the average plantation soil.

The record shows that it is not confined to any particular district; most of the cases come from the low country, but two are from an elevation of about 2000 feet. The fungus has been found, as a saprophyte, in various districts up to an elevation of 2000 feet.

The mycelium of the fungus at once distinguishes this disease from those caused by *Fomes semitostus* and *Hymenochæte noxia*. When the root is dug up the cortex is found to be decayed, but there is no external mycelium. But if the cortex is removed, black or red flattened strands are found running over the surface of the wood (see Plate 3). These are usually about 2 mm. in breadth, but they may be as much as 5 mm. At first they are red externally and white internally, but when the root is much decayed the strands decay also and become black. If the fungus is living the strands are fairly thick and stand out prominently from the wood, but in decayed specimens they are little more than a black film on its surface. In some cases these strands run in the cortex, beneath the thin, outer papery layer which peels off from the dead root.

The figure on Plate 3 indicates also the manner in which it usually attacks a tree. The mycelium enters the finer roots, and advances between the wood and the cortex until it reaches the larger roots. There it spreads out and branches, and these branches advance further until the tap-root is reached. On the tap-root it forms similar strands, but at the collar it may form a continuous red sheet, between the wood and the cortex as before. The point on the figure from which the strands radiate is the place where a small root, about 2 mm. in diameter, joined the larger root.

Fig. 7 on Plate 4 shows a stronger development of mycelium. In this case the strands are much broader, and they have fused behind into a continuous sheet. The outer surface of the strands is marked with a peculiar herring-bone pattern; this occurs also on the narrower strands in *Hevea*; but it is not so easily recognised there. This particular specimen was found between the wood and the bark of a decaying dadap log. When the bark was torn from the wood these thick strands were often split horizontally, and the white internal tissue then exhibited a fern-like appearance. The patch of mycelium was over a yard in length, and about eight inches broad; the separate strands were evident only at the margin.

In roots attacked by this fungus the outer wood is deep blue when fresh, but the colour fades when they become dry. This symptom is, however, not peculiar to this disease.

When the decayed bark cracks, either on exposed roots or at the base of the stem, and exposes the mycelium, the latter produces fructifications. These are of two kinds, a conidial form, in which the spores are borne free upon the apex of short stems, and an ascigerous form, in which the spores are enclosed in sacks (asci) within a small case (perithecium). The complete fungus was named *Sphærostilbe repens* by Berkeley and Broome in 1875.

The conidial fructifications are the first to appear. They consist of short erect red stalks, from 2 to 8 mm. high ($\frac{1}{12}$ to $\frac{1}{3}$ of an inch), and 0·5 to 1 mm. in diameter ($\frac{1}{50}$ to $\frac{1}{25}$ of an inch), surmounted by a white globose head, 1 to 1·5 mm. in diameter. Though individually small, they occur in large numbers, and are therefore fairly conspicuous. The stalk is hairy at first, but subsequently becomes glabrous in the lower half; it is composed of parallel hyphæ, which separate in a brush at the apex, and then bear the spores (conidia) along their sides.

There is a very common conidial fructification which might at first sight be mistaken for that of *Sphærostilbe repens*. It is often found on dead *Hevea* and dead cacao, and frequently occurs on the pruned ends of the branches of rose bushes. It grows in large clusters like a number of small red pins. But it is easily distinguished even with a hand lens, since its stalk is smooth, not hairy. It is the conidial stage of *Megalonectria pseudotrichia*, a very common saprophyte, which may do some damage when it grows on the pruned ends of rose branches.

When the conidial form of *Sphærostilbe repens* has matured its spores, the second form makes its appearance. The perithecia are small dark red bodies, rounded below and conical above, about 0·6 mm. high ($\frac{6}{250}$ of an inch) and 0·4 mm. in diameter ($\frac{4}{250}$ of an inch). They are crowded together along the edge of the mycelium (stroma), if that is exposed, or round the bases of the old stalks, or even along the stalks. Each perithecium contains a number of elongated sacks (asci), and each ascus contains eight spores. When the spores are ripe, they are shot out through an orifice at the apex of the perithecium. (For measurements and further details see later).

Sometimes the creeping strands of mycelium (rhizomorphs) become superficial, especially when the fungus is only saprophytic; the conidiophores and perithecia are then borne anywhere along the strands.

The perithecia of *Sphærostilbe* are identical in structure and appearance with those of a *Nectria*, but as the conidial stage differs from that of a *Nectria*, it is placed in a different genus.

Sphærostilbe repens was first collected by Thwaites, at Peradeniya, about the year 1868. His specimens were found on jak (*Artocarpus integrifolia*). Whether it was parasitic or saprophytic is not recorded, but judging from later occurrences it was probably only saprophytic. In 1908 a healthy jak tree was felled in the Botanic Gardens, and soon afterwards *Sphærostilbe repens* developed in abundance on the chips which were left lying round the base of the stump.

It was found in 1906 on a decaying dadap log about a foot in diameter. The tree was quite sound when felled; so that the fungus in this case also was only saprophytic.

Specimens were sent in in 1906 on the rhizome of arrowroot (*Maranta arundinacea*, L.). In this case it was undoubtedly parasitic. The rhizomorphs had apparently spread through the soil to the arrowroot, and, after growing for some distance along the scale leaves, had penetrated the rhizome, where they formed a number of more or less parallel strands running lengthwise through it. The rhizome was decayed along one side, but in general the tissue surrounding the rhizomorphs appeared quite sound. The extension of the rhizomorphs would appear to depend on the supply of food behind, not on food obtained from the immediately surrounding tissues, at least in the early stages of extension.

Conidia (*i.e.*, the first kind of spore), sown on cut surfaces of arrowroot, developed the first stage of the fungus in seven days, and the second stage in twenty-one days. No experiments were made with the ascospores, but in all probability both kinds of spore are equally effective in spreading the disease.

As previously stated, the fungus develops freely on small pieces of jak wood, and therefore may be expected to occur on or around jak stumps. In this respect it resembles *Fomes semitostus*, and provides an additional reason for getting rid of decaying jak stumps. In the first case recorded the fungus was most probably introduced with firewood; it should scarcely be necessary to point out that it is not advisable to store firewood round or near *Hevea* trees.

From the fact that it is parasitic on arrowroot, the fungus may be expected to attack other plants of the same order, *e.g.*, plantains, cardamoms, turmeric, ginger, and the many related species which occur in tropical jungles. It is not probable, however, that these will serve as sources of infection for *Hevea* plantations.

Dead trees must be dug up and burnt, as much of the roots as possible being extracted. Any neighbouring stump, especially if a jak stump, should be dug up, and all pieces of wood collected and burnt. Small pieces of wood are quite sufficient to provide a suitable habitat for the fungus. Since it can spread through the soil to adjacent trees by means of its rhizomorphs, the affected area must be surrounded by a trench about a foot in depth; the ground enclosed within the trench should be dug over and liberally dosed with quick-lime, which should then be forked in.

Sphærostilbe repens, B. and Br.—Conidiophores, 2–8 mm. high, 0·5–1 mm. diameter; stalk pink at first, then red-brown, tomentose, becoming glabrous at the base later; head translucent at first, then white and opaque, globose 0·5–1 mm. diameter; the hairs on the stalk are 8–10 μ in diameter, septate, with septa 20–30 μ apart, strongly constricted at the septa, and densely spinulose. Conidiferous hyphæ, 3 μ diameter, septate, bearing conidia on short, blunt processes, one below each septum; unbranched in the upper 200 μ. Conidia variable, broadly oval or narrow oval, apiculate at one end, or rounded at both ends, hyaline, 9–22 × 6–10 μ. When the spores are mature, the whole head separates from the stalk if touched with a needle, leaving a flat, white disc at the top of the stalk.

Perithecia clustered, along the edge of the stroma, or

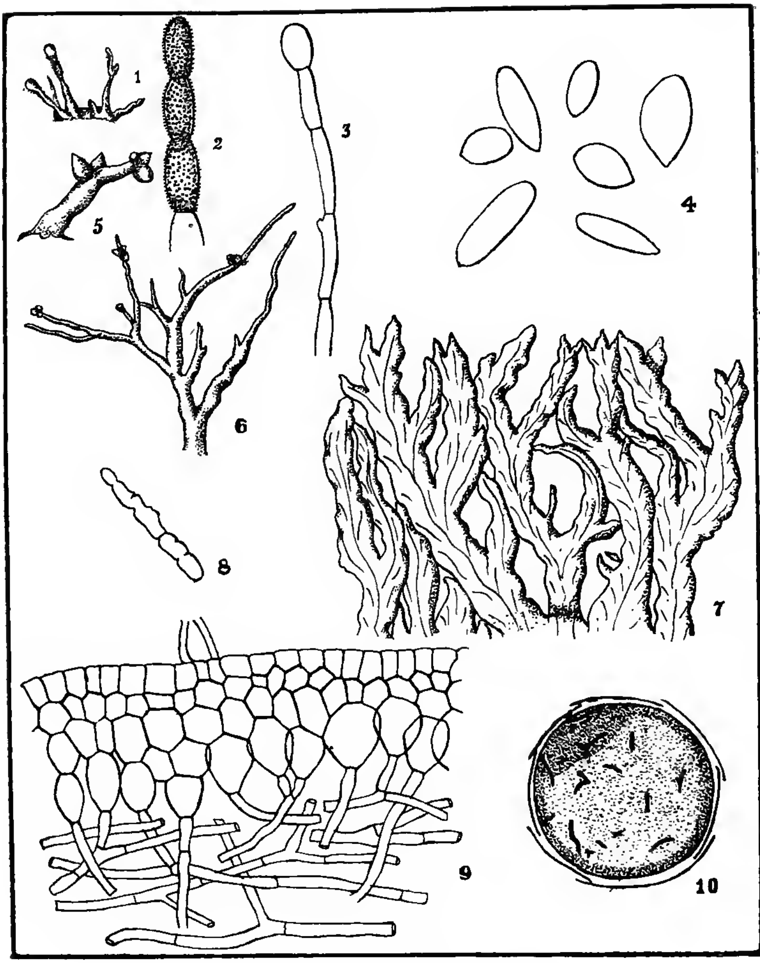

Plate IV.—SPHÆROSTILBE REPENS.

Details of the fungus.

round the bases of the conidiophores, or elevated along the conidiophores, dark red, slightly rough, about 0·6 mm. high and 0·4 mm. diameter, rounded below, conical above; asci cylindric, 190–220 × 9 μ, apex rounded and thickened, eight-spored, spores uniseriate. Paraphyses wanting. Spores one-septate, generally two-guttulate, oval, slightly constricted, pale brown or reddish-brown, 19–21 × 8 μ.

Mycelium forming red-brown flattened rhizomorphs, 2 mm.–1 cm. broad, and 0·5–2 mm. thick, which sometimes coalesce into a continuous sheet; rhizomorphs marked on both surfaces by an almost continuous median groove and short oblique lateral grooves. Internally the rhizomorphs are white, and consist of loosely interwoven hyphæ, 4–8 μ diameter, running more or less longitudinally. The cortex is about 100 μ thick, and consists of four or six layers of rounded or polygonal 'cells,' 10–15 μ in diameter. The outer layer is brown, the next one or two yellow-brown, and the inner two or three hyaline; 'intercellular spaces' are present in the inner layers only. This cortex appears to be formed by special lateral branches of the internal hyphæ, which produce at their extremities branching chains of oval cells. The outer surface appears glabrous, but gives off a few hyphæ, which penetrate the surrounding tissues.

In some respects this species resembles *Corallomyces*, but the perithecia are in general situated on the sessile stroma.

Explanation of Plate 4.

Fig. 1. A group of conidiophores. × 2.
Fig. 2. Tip of a hair from the stalk. × 350.
Fig. 3. Conidiferous hypha from the apex of the conidiophore. × 500.
Fig. 4. Conidia. × 600.
Fig. 5. Perithecia on an old conidiophore. × 6.
Fig. 6. Groups of perithecia on a horizontal rhizomorph. × 2.
Fig. 7. Rhizomorphs (advancing edge of the stroma) from *Erythrina*.
Fig. 8. Cross section of a rhizomorph, outline only. × 2.
Fig. 9. Cross section of the cortex of a rhizomorph.
Fig. 10. Cross section through the rhizome of arrowroot attacked by *Sphærostilbe*. The dark lines are the sections of the rhizomorphs.

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Miscellanea.

In 1901 a sterile mycelium, found on the roots of a *Hevea* in Malaya, which was said to be the cause of a root disease, was assigned to a species of *Helicobasidium*, probably *Helicobasidium mompa.* Nothing further has been heard of this disease. As Patouillard has already pointed out, no species of *Helicobasidium* has been proved to be parasitic; all the known species are saprophytic, or epiphytic, as far as the plant on which they are found is concerned. Until further evidence is forthcoming this name should be deleted from the list of *Hevea* parasites; there is no reason to believe the mycelium, even if parasitic, was that of a *Helicobasidium.*

Irpex flava has also been recorded as the cause of root disease in *Hevea.* This fungus is one of the commonest in the tropics, and, in my experience, is never more than saprophytic. As a saprophyte, it is common on dead bamboos, and generally adorns those which are used as pea-sticks, &c. It was said to be a root parasite in the days of coffee, thirty years ago, but in view of its present status that statement must be doubted.

In addition to the root diseases already described, there is probably another in Ceylon; but only two specimens have been obtained up to the present, and it is as yet doubtful whether the fungus observed is really parasitic. The root is covered with black strands of mycelium, which recall those of *Rosellinia*; but they lack the microscopic characters of a *Rosellinia*, and the diseased roots have developed, not a *Rosellinia*, but a *Xylaria*, viz., *Xylaria Zeylanica*, Berk. Further investigation is necessary before any decision can be arrived at.

CHAPTER XI.

STEM DISEASES.

FOUR stem diseases of *Hevea* have been recorded, viz., 'canker,' caused by *Phytophthora Faberi;* 'pink disease,' caused by *Corticium salmonicolor* (= *Corticium javanicum*); 'dieback,' in which the most destructive agent is *Botryodiplodia theobromæ;* and a 'black canker, caused by a species of *Fusicladium.* Of these, pink disease is readily distinguished by the pink film upon the stem. Dieback first affects the top of the tree and gradually kills it back to the base; it might be confused with root disease, but not with the other stem diseases. Canker kills the cortex locally, but does not give much outward indication; the bark is often darker than normal bark, and it may exude a reddish liquid; but this disease is more frequently discovered during tapping than by inspection of the trees. The *Fusicladium* canker apparently resembles dieback closely, but few particulars are to hand.

In addition to the above, two stem diseases occur on young plants; and another, which attacks old and young plants alike, is recorded here for the first time.

CANKER (*PHYTOPHTHORA FABERI*, MAUBL.).

The popular name of this disease is a misnomer. The term 'canker,' as generally understood, conveys the idea of an open wound surrounded by a callus which ultimately becomes gnarled and irregular. Nothing of the kind occurs in *Hevea* canker, as a rule. The diseased bark is usually quite smooth, and often to all outward appearance quite sound; it does not fall off and leave open wounds. The tree may be killed and the whole of its bark decayed, without the occurrence of any roughness or open wounds. Many cases of pink disease merit the name of 'canker' far more than those of *Phytophthora Faberi.* However, as the name has been definitely attached to a well-known disease, nothing would be gained by trying to change it; an attempt to do so in 1905 led to the announcement that there was no disease of any kind attacking *Hevea* stems!

This disease appeared on *Hevea* in Ceylon about the year 1903. During 1903-4 it was investigated by Carruthers, who decided that it was caused by a red *Nectria.* The disease is in many respects strikingly similar to that of cacao 'canker,' which was also attributed to the agency of a red *Nectria*, but since the red *Nectria* which occurs on diseased cacao stems is a different species from that which grows on diseased *Hevea*, it was decided that there was no connection between the two diseases. Carruthers (in litt.) stated: 'The cacao canker (*Nectria*) is not the same fungus as that which attacks the rubber (*Hevea*), and there is therefore no danger in its being near rubber trees.'

Since then it has been proved that cacao canker is not caused by a *Nectria*, but by *Phytophthora Faberi*, which has been for several years recognised as the cause of cacao pod disease; the proof of this is due to Rorer, of Trinidad, though Busse had stated several years previously that the *Phytophthora* grew on the bark as well as on the pod. Similarly, it has been shown that the canker of *Hevea* is also caused by the same *Phytophthora*, while all inoculations with the various stages of the *Nectria* have been unsuccessful. Both on cacao and *Hevea* the *Nectrias* found on cankered trees are harmless saprophytes. Both cankers are caused by the same fungus, *Phytophthora Faberi*, which is also the cause of cacao pod disease and *Hevea* pod disease.

The external symptoms of canker on *Hevea* are by no means so clear as on cacao. On young trees the bark may appear darker, but where the tree has acquired a thick outer brown bark there is practically no outward indication. In some cases the bark exudes a reddish or purplish liquid; in very wet weather this occurs when only a small patch of bark is diseased, but under ordinary conditions, it only happens when a large area is affected. In many cases the disease has only been discovered by observing that the tree suddenly ceased to yield latex. Whenever this happens the bark should be lightly scraped here and there to

Plate V.—*HEVEA* CANKER.

Caused by *Phytophthora Faberi*;
cortex cut away to show the discoloured area.

see whether it is discoloured internally. Of course, if the disease is not discovered by the planter at an early stage, the decaying bark soon attracts a multitude of boring beetles, and their depredations direct attention to the tree.

Healthy *Hevea* cortex is white, or yellowish, or clear red, or sometimes mottled red and white, internally; and when the outer layer of brown bark is scraped off, a green layer is found overlying the laticiferous tissue, at least on trees which have not acquired scaly bark. But when the bark is 'cankered,' a black layer is found beneath the outer brown bark, and underneath this the cortex is evidently discoloured (Plate 5). When recently diseased it is greyish, or 'neutral tint,' with a well-defined black border, but in advanced cases it becomes claret-coloured or purple-red, 'not unlike the inside of the fruit wall of a ripe mangosteen.' Frequently the diseased cortex is dirty red when cut, but darkens to purple red soon after exposure.

No latex exudes when cankered bark is cut. Any area which does not yield latex when pricked with a penknife should therefore be scraped here and there to see whether the layer beneath the brown bark is still green or whether the cortex is discoloured. But pricking the bark is not a reliable test for 'canker'; for the outer layers may be diseased while the inner layers are healthy, and in that case latex will flow from the inner tissues when the bark is pricked, although it may be diseased halfway through.

The disease is often discovered by the cessation of the latex flow. Sometimes all the incisions, sometimes one or two only, yield no latex when tapped. In some cases the incision cuts across a patch of cankered bark, and the disease is clearly evident; in other cases the diseased patches occur below or between the tapping incisions, and are not found until the bark is scraped.

Phytophthora Faberi, the cause of cacao and *Hevea* canker, belongs to the same family as the fungus which causes one of the most serious potato diseases. It flourishes better on cacao pods than on cacao or *Hevea* stems; indeed, it may be said that its occurrence on the latter is quite at variance with the hitherto accepted ideas as to the habitat of a *Phytophthora*. On cacao pods its mycelium is at first internal, but it may subsequently form a white covering of 'mould' over the whole surface; on the other hand, on cacao or *Hevea* stems it is almost entirely internal, the external growth being so small that it has generally been overlooked.

The mycelium bears minute egg-shaped bodies, usually provided with a papilla at the apex. These are known as sporangia. They are easily detached from the hyphæ, and can therefore be distributed by the wind. When a sporangium falls into a drop of water its contents divide into a number of spores; these escape through the apex of the sporangium and swim about in the water by means of the two cilia which each possesses. These spores are known as zoospores. After a short time they come to rest, round themselves off, and germinate in the same way as ordinary spores. In some cases the sporangia are spherical instead of egg-shaped; apparently these do not produce zoospores, but behave as ordinary spores. The sporangia soon die if exposed to sunlight.

In addition to the sporangia, thick-walled spores are produced, either among the external mycelium or within the tissues of the host plant. They may be found in the cacao pod wall, but I have not seen them in *Hevea* cortex. These are known as oospores; they are more resistant than the zoospores, and while the latter provide for the rapid dissemination of the fungus in wet weather, the former serve to carry it over the dry season. Those within the tissues of the host plant can only be liberated by the decay of the latter.

It follows from our knowledge of the life history of the fungus that the stems of *Hevea* are most liable to infection when they are wet, for the sporangia cannot produce zoospores in the absence of moisture. Attention has already been directed to the effect of different systems of planting upon the protection afforded to the stems by the crown of the tree. It is quite clear that, in *Hevea*, 'canker' is produced by spores which alight on the stem, and, after germination, produce a mycelium which gradually destroys the cortex from without inwards; for in the early stages of the disease the discoloration begins beneath the outer layer and does not extend to the cambium. The spores, blown by the wind, lodge on the rough bark, and if the stem is sufficiently moist a canker patch is produced at the end of a few weeks. Frequently several patches are found on the same stem, each the result of a separate infection ; this may be due in part to the fact that it is the sporangia which are carried by the wind, and each sporangium produces several zoospores, which may be washed by the rain to different parts of the stem.

Excision of all the discoloured tissue is the recognised treatment for canker. It is not necessary to cut out more than the discoloured area, because the fungus threads have not advanced beyond it. All the excised tissue must be removed and burnt. The difficulty here is the discovery of the canker before it has progressed so far that a large area has to be excised. The tapping coolies should be shown what cankered bark is like, and they should be instructed to stop tapping and report any trees which cease to yield latex, even if the flow ceases only on one cut. Many cases of canker are only discovered by the cessation of the latex flow, and it is not uncommon to find that the coolie has been tapping for weeks on cankered bark from which he could not possibly have obtained a drop of latex.

If the wounds caused by the excision of cankered bark are small, cow-dung and clay is the best covering which can be used to promote the healing process. But where they are large, so that the cortex cannot be expected to grow over them, the exposed wood must be protected. If it is left unprotected it is soon riddled by boring beetles, which rapidly bring about the destruction of the tree. I would suggest that the exposed wood be tarred, except for a strip of an inch all round, and that this strip be treated with cow-dung and clay as in other cases.

If a moderately thin slice of 'cankered' bark is gradually broken by bending it while still moist, so that the break is not too sudden, strands of rubber will be found stretching across the crack. The rubber is not consumed by the fungus, as has been suggested, but is coagulated in the latex tubes, probably by the substances produced by the fungus in its growth.

Cracked, scaly bark is not necessarily a sign of canker in *Hevea*. In the majority of cases of canker the bark is not cracked. The formation of scales which can be easily detached from the stem appears to be a normal phenomenon in old *Hevea* trees, as it is in old jak trees, and it is apparently induced prematurely by tapping. But quite young trees may develop scaly patches without any discoverable cause. When these scales can be detached easily, and leave sound laticiferous tissue, usually covered with a brown film, on the stem, there is no 'canker.'

During the prolonged rains of 1909 and 1910 the renewing bark on the tapped surfaces decayed in many cases. The decay was first indicated by the appearance of vertical black lines just above the tapping cut. When the bark was cut out it was found that these lines extended into the wood, and that they were present on the cambium before they were visible externally. The bark along these lines rotted, and left long, narrow wounds extending down to the wood. In some cases the decay travelled downwards and involved the untapped bark also. When the rains ceased the decay stopped and the wounds healed up, but the renewal was, of course, rough with vertical swollen ridges of wound tissue.

This decay does not appear to be due to 'canker.' The colour of the diseased bark differs, and the decay ceases when fine weather sets in. Latex can be obtained from the affected tapping cuts, as there are strips of sound bark between the black lines. It does not therefore seem advisable to stop tapping when this occurs, unless it can be shown that it is due to some organism which can be conveyed from tree to tree by the tapping knife. Up to the present, all attempts to reproduce this decay by means of the organisms found in the decayed bark have failed, and it seems probable that it is due only to an excess of moisture on the layers exposed during tapping. It is scarcely worth while to excise these black patches, because the amount of injury caused by excision is greater than that caused by the decay of the bark. Some years ago there was put on the market a 'latex protector,' *i.e.*, a collar of metal which could be fixed round the tree above the tapped area, so as to prevent the rain-water flowing down the stem over the tapping cuts and into the collecting vessels. It would be worth while to experiment with some similar device in cases where this decay of the tapped surface is prevalent.

As in the case of cacao, *Phytophthora Faberi* attacks both the stem and the fruit. The disease of the fruits is worst in exceptionally wet seasons, and it disappears when the rains cease. It has not attracted so much attention of late years, now that the demand for seed has diminished. The pods turn black, not a clear black, such as may be produced by 'black blight' growing over them, but a sodden, watery discoloration. They rot on the tree, the outer soft layer ultimately shrivelling and splitting, but the fruit does not dehisce and the seeds are not liberated. This was determined to be due to *Phytophthora* in 1905. In that year the disease threatened to destroy the whole crop, but it ceased when fine weather set in. The soft green tissue which covers the woody wall of the seed capsule is very thin, and it does not appear to afford a suitable habitat for the fungus except under very favourable weather conditions. As the fruits do not grow on the main stem, the fungus does not travel into the latter and produce canker there as it does in cacao; it may grow through the stalk into the green branch, and kill that for some distance, but it has not been found to proceed further. Since the fruits are now of little value, the only danger in the fruit disease is that the diseased pods may serve as a source of infection for the stem canker. The experience of pure rubber, or rubber and tea estates, would seem to show that this danger is small; for 'canker' has not proved such a serious disease on them as it was thought to be in 1904. The advice given in 1905, that diseased fruits should be collected and burnt, should be followed when the fruit disease is serious. It is the only possible measure, and it may prevent a certain amount of stem 'canker.'

Green *Hevea* shoots usually develop black patches or a continuous black coat. This is due to a black fungus, *Meliola* or *Asterina*, &c., which is purely superficial. At the base of each *Hevea* leaf there are two nectaries which secrete a sugary fluid, and the black fungus lives on this just as it does on the secretions of insects. This black film occurs on the fruits also. It is in no way connected with the *Phytophthora* disease, and it does not destroy the tissues beneath it. If it is scraped, the underlying tissues will be found to be green and full of latex.

When *Hevea* is interplanted among cacao, the cacao pods serve as a continual source of infection for the *Hevea*. Moreover, the dense shade of the mixed plantation favours the growth of the fungus on both products. The following is cited as an example of the prevalence of the disease on a mixed estate, and is in striking contrast to the comparative absence of canker on estates in rubber only. The *Hevea* is seven to eight years old, and only the trees which had come into tapping had been examined when this table was drawn up.

Field.	Acres.	Number of trees tapped.	Number diseased.	Percentage.
A	16	1398	94	6·7
B	12½	802	51	6·3
C	16½	425	10	2·4
D	9½	1119	17	1·5
E	6	457	1	0·22

The *Hevea* is planted, in some cases 15 feet by 15 feet, and in others 15 feet by 20 feet. Fields A and B also bear old cacao planted 7½ feet by 7½ feet; fields C and D are similarly planted, but the cacao is young; and field E is in tea, surrounded by the cacao and rubber fields.

The effect of 'canker' on *Hevea* is more serious than on cacao. In cacao, although the stem is shaved periodically, the tree continues to yield a crop until it is killed by complete ringing. But, as the bark is the source of revenue in *Hevea*, it is impossible to be always cutting it away in the same manner. When the whole of the bark on one side has to be cut away for a length of two or three feet, that tree is useless for tapping for several months, and in all probablility that area will never be tappable again.

Therefore, since periodic treatment of the same tree is out of the question, the chief measures adopted must be preventive; and, as we know that the cacao is a permanent source of infection, and that canker is almost negligible on purely rubber estates, there is no doubt that the chief preventive measure on a mixed estate must be the removal of the cacao. When canker attacks *Hevea* which is interplanted among cacao, the cacao must be sacrificed if the *Hevea* is retained as a paying crop.

The removal of the cacao must be effected gradually. Where the cacao is planted 7½ feet by 7½ feet, the sudden removal of all of it would be liable to injure *Hevea* which had been accustomed to so much bottom shade and consequent humidity. The cacao in the rows of *Hevea* might be taken out first, and the balance after six months or so. The cacao must be uprooted, not merely felled. If the stumps are left in the ground they will develop *Hymenochæte noxia*, which will spread from them to the *Hevea* and kill it. This is being fully demonstrated on estates where alternate lines of cacao have been cut out to make room for *Hevea*.

The disposal of the cacao stems and branches is a problem which does not admit of easy solution. Once the trees are uprooted they are not likely to develop either *Phytophthora Faberi* or *Hymenochæte noxia*. But they will certainly develop *Botryodiplodia theobromæ*, which can attack *Hevea* as a wound parasite, and is the serious fungus in 'dieback.' It is probable that this might be risked; but the safest plan is to burn all the cacao débris as soon as possible.

When estates are seriously attacked by 'canker,' it would be worth while to spray the trunks with Bordeaux mixture just before the monsoon rains set in. This would kill the spores which might subsequently fall on the stems, and prevent further infection during the wet weather. But it must be remembered that spraying will not cure a diseased tree. If the tree is 'cankered' the diseased bark must be cut out.

Up to the present time, *Hevea* 'canker,' caused by *Phytophthora Faberi*, has been recorded only from Ceylon and South India, though it would seem from the accounts of other diseases which are published from time to time that it occurs in other countries but is confused with 'pink disease.' There is no doubt that it will be found in other cacao-growing countries, since the pod disease and stem canker of cacao are caused by this fungus all round the world.

Phytophthora Faberi, Maubl.—Mycelium richly branched, both inter- and intra-cellular, unseptate in the earlier stages of growth, but later forming septa, which frequently serve to separate off empty and dead portions of the mycelium. Haustoria not observed. Sporangia generally ovate, but somewhat variable in shape. Extreme measurements as given by Von Faber are $25 \times 30\ \mu$ and $42 \times 80\ \mu$. Sporangiophores in water culture, sympodially branched, as in *Phytophthora omnivora*, and bearing under favourable conditions up to 20 sporangia or more. Oogonia formed intramatrically in the host plant, as well as in artificial cultures on agar and sterilised potato. Antheridia absent, or if at all present, only rarely formed. Oospores entirely, or for the most part, formed parthenogenetically, occupying practically completely the oogonial cavity, in artificial culture spherical, in the host tissue spherical, or elongate, or irregular, 22–45 μ diameter. (Coleman, in *Annales Mycologici*, vol. viii., pp. 621, 622).

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PINK DISEASE.

CORTICIUM SALMONICOLOR, B. & BR. = *CORTICIUM JAVANICUM*, ZIMM.

This disease was discovered by Zimmerman in Java, where it attacks cultivated plants of all kinds, and is apparently a more serious parasite than in Ceylon. He published a description of the fungus under the name *Corticium javanicum*. In 1904 the discovery of a pink fungus on *Hevea* bark was announced in the Federated Malay States, and in 1906 it was said to be *Corticium calceum*; the latter is a European species which has never been accused of causing any damage, and this misidentification was the more unfortunate since it deprived the Federated Malay States planter of the information which had already been accumulated with regard to this disease in Java. In 1906 it was found to attack *Hevea* in Ceylon, and was recorded under the name of *Corticium javanicum*; and shortly afterwards specimens were forwarded from Southern India. In 1909 it was again recorded from the Federated Malay States, this time under the name of *Corticium Zimmermanni*. In the West Indies a similar fungus on cacao has been referred to *Corticium lilacofuscum* (? C. *lilacino-fuscum*, B. & C.), but as it is known under the name of pink disease it would seem probable that this is another error in identification.

One fact emerges from all these records, viz., that in all tropical countries there is a stem disease characterised by the presence of a thin pink film overlying the bark. There is little doubt that these diseases are caused by the same fungus in all cases, but they have been made to appear different by giving the fungus different names in different countries. Two of these names are errors, *i.e.*, they are the names of other fungi; the other two are synonyms, *i.e.*, they are different names for the same fungus. Zimmermann called his fungus *Corticium javanicum*, but the name was afterwards changed to *Corticium Zimmermanni* by Sydow and Saccardo; this change was founded on a series of mistakes, and is contrary to the rules of botanical nomenclature.

However, further investigation has shown that the fungus was named and described before Zimmermann discovered it. Among the fungi forwarded from Ceylon by Thwaites in 1870 was one which was named *Corticium salmonicolor* by Berkeley and Broome. As there was no specimen of this in the Peradeniya herbarium it was impossible to make any comparison with *Corticium javanicum* until recently, but inspection of the type specimen at Kew has shown that it is identical with the latter species. Berkeley and Broome's name must therefore be adopted for this fungus.

In Ceylon this disease attacks *Hevea*, tea, cinchona, plum, orange, and coffee. In Java, where it is known as 'Djamoer Oepas,' it attacks coffee, ramie, cacao, cinchona, nutmeg, tea, *Eriodendron*, pepper, coca, cinnamon, Kola, *Castilloa elastica*, *Hevea*, dadap, *Bixa orellana*, mango, and many other trees or shrubs of minor importance. Ridley states that it occurs in the Straits on ramie and *Strobilanthes* when overcrowded and too damp. In South India it has attacked *Crotalaria* interplanted among *Hevea* in districts where the former has grown to a great height, and has developed stems up to an inch in diameter.

In *Hevea brasiliensis* the disease generally originates at the fork of a tree, or where several branches arise close together from the main stem. The first sign of it is the appearance of a pink incrustation of interwoven hyphæ over the bark. This pink patch gradually extends, and may ultimately cover the whole circumference of the tree and the bases of the adjacent branches for a length of several feet. Under the central parts of the patch the bark is dead, and usually dry, but towards the margin the fungus is superficial. When once the fungus is established, it travels over the bark faster than within it; hence, although the whole of the bark is permeated by the fungus over the greater part of the patch, the advancing margin is generally superficial. The dead bark usually dries up, cracks, and splits away from the wood.

The fungus does not enter the wood to any appreciable extent, and the pink fructification only occurs on the bark. Its spread is governed largely by the conditions of moisture or exposure; if, for example, the rain water runs down one side of the tree only, the fungus may be confined to that side; and similarly it will exhibit greater growth on the more shaded side. The amount of damage done before the disease is noticed depends upon the size of the tree; young stems about two years old are quickly encircled and ringed, but on older trees the patch should be detected before it has grown completely round the tree.

The pink patch is extremely thin, and, when old, splits everywhere in lines more or less at right angles to one another. For this reason it has been called the 'writing fungus' in Malaya, the fragments being thought to resemble hieroglyphics. Old specimens lose their pink colour and become ochraceous, or, when very old, are bleached to white. In some instances, in very wet weather, even young, recently developed patches may be white; and under the same conditions, the bark may be covered with long silky hyphæ instead of the usual interwoven felt.

During the monsoon rains, the fungus, if not attended to, grows continuously, and kills off the bark uniformly. The side branches at the point of attack are ringed and thus killed, and the bark of the main stem peels off in large patches. Between the dead bark and the wood, insects of various kinds are found, but these have merely taken refuge under the loose bark, and are not responsible for any of the damage. Young trees may be ringed, and the whole of the tree above the point of injury may die, in a single rainy season. But in many cases the disease has not advanced far enough to kill the tree by the time the rains cease. The fungus then stops growing when the dry weather sets in, after having killed off part of the cortex and cambium of the main stem, and probably some of the side branches also. This leaves an open wound over which there is no cambium to produce new wood and cortex. Consequently there is formed a 'canker' in the true sense of the term, *i.e.*, an open wound, exposing the wood, surrounded by a swollen callus. Large open wounds on the upper part of the stem are generally the result of the attacks of *Corticium salmonicolor*.

In the majority of cases the pink incrustation is always present, and there is no difficulty in diagnosing the disease.

Where the growth of the fungus has been checked by the dry weather and a 'canker' has been formed, the incrustation may have weathered off, or have been thrown off with the dead bark; but it is usually possible, even in such cases, to find traces of the pink patch on the fragments of dead bark which persist round the wound.

The disease is conveyed from tree to tree by spores which are blown by the wind. From the time of attack, it is most probable that the fungus develops in the jungle on native trees at the beginning of the monsoon and then produces spores which blow into the neighbouring plantations. For example, on one Ceylon tea estate the attack always occurs towards the close of the monsoon. In the dry weather it probably exists in a vegetative state; it is scarcely probable that the thin-walled spores would survive the dry season. The disease begins in the fork of a tree, or where several branches arise more or less in a whorl from the main stem, because such places are kept damp by the rain water which flows over them and afford favourable conditions for the germination of the spores. When the spores germinate, they emit hyphæ which penetrate into the cortex and for some time grow entirely inside it. Then small cushions of pink fungus tissue appear in minute cracks in the bark, and from these cushions the pink felt spreads over the exterior. The pink patch, which is at first composed of interwoven hyphæ adpressed to the bark, soon develops short cylindrical cells (basidia) closely packed together, standing perpendicularly to the basal hyphæ, and each of these basidia bears four minute spores at its apex. The spores are thus produced on the surface of the pink patch, ready to be blown elsewhere at the first opportunity. It may be noted that these spores are formed directly the patch is fully developed, *i.e.*, during or at the close of the wet season; they are not likely to be produced on old patches during the dry weather.

There is no reason whatever which would lead one to suppose that *Corticium salmonicolor* is a wound parasite. All the evidence goes to prove that it can attack uninjured trees.

When the disease attacks young trees one to two years old, they should be cut back below the affected part. The treatment of older trees must depend on the extent to which the tree is attacked; in many cases it will be possible to cut away the diseased bark only, if it is discovered before the fungus has grown completely round the tree; but if the injury is extensive, the diseased stem must be cut off in these cases also. The wounds caused by excising the diseased bark or pruning off the affected stem should be tarred; and all the diseased tissues should be burnt. It is stated that the disease has been checked by tarring the affected bark, and also by washing it with copper sulphate. Since the fungus is in part superficial, there is no doubt that much of it would be killed by either of these processes, but since the diseased bark will undoubtedly die (if it is not already dead) and scale off, and thus remove the preservative or fungicide used, it is far preferable to cut out the diseased bark at first and then protect the wound by tarring.

In 1909 'pink disease' caused serious loss on many estates in South India by killing off large numbers of young trees, and it was recommended that they should be sprayed with Bordeaux mixture just before the monsoon in order to prevent infection during the ensuing wet weather. This treatment has been carried out on one estate, and details of the cost have been published by Anstead in the *Planters' Chronicle*, May 21st, 1910. The mixture was applied with a brush, and painted on where the branches joined the main stem. The cost of treating 500 acres was Rs. 150, but 200 acres consisted of young trees, 2½ years old, and cost very little. It would certainly pay to adopt this treatment on areas where 'pink disease' reappears periodically. As a rule, this disease in Ceylon is not spread over the whole estate, but occurs in certain patches. If the trees on fields which are known to be liable to infection were treated in the manner indicated prior to the south-west monsoon much loss would be prevented. While the disease does not cause any widespread damage in Ceylon, the trees attacked are usually six to eight years old, and it is certainly worth while to make some attempt to save these.

Further particulars have been given in the *Planters' Chronicle*, October 1910. It is there stated that 'up to date the treatment has resulted in complete success, and the cases of attack have been reduced to a few individual instances, and these are probably due to the careless application of Bordeaux. Thus in one instance out of 60,000 treated trees there have so far been only three cases of pink disease where formerly there would have been hundreds. On estates where Bordeaux mixture has not been used, and which therefore act as a check, the disease has been as bad as usual, and attacked trees may be put down roughly at something like one per cent.' The cost is said to be half a pie per tree.

The application of Bordeaux mixture with a brush is practicable in such a case as the present, and of course that method obviates any large initial outlay on sprayers. But it may be doubted whether it is really cheaper than spraying in the end. It has been found, in Southern India, that the coolie cannot, or will not, work a sprayer if he has to pump with one hand and direct the spray with the other. The same thing happened on the Experiment Station, Gangaruwa; and for that reason a sprayer worked by compressed air was adopted. But where the ground is level barrel pumps are preferable to knapsack sprayers; and they might be used on any estate if fitted with poles by which they could be carried.

The following method of making Bordeaux mixture was given by Professor B. T. Galloway, of the United States Department of Agriculture:—

'All things considered, it is believed that the best results will be obtained from the use of what is known as the 50-gallon formula of this preparation. This contains:

Water	50 gallons.
Copper sulphate	6 pounds.
Lime...	4 pounds.

'In a barrel or other suitable vessel place 25 gallons of water. Weigh out 6 pounds of copper sulphate, then tie the same in a piece of coarse gunny sack, and suspend it just beneath the surface of the water. By tying the bag to a stick laid across the top of the barrel no further attention will be required. In another vessel slack 4 pounds of lime, using care to obtain a smooth paste, free from grit and small lumps. To accomplish this it is best to place the lime in an ordinary water-pail, and add only a small quantity of water at first, say a quart or a quart and a half. When the lime begins to crack and crumble, and the water to disappear, add another quart or more, exercising care that the lime at no time gets too dry. Towards the last considerable water will be required, but if added carefully and slowly a perfectly smooth paste will be obtained, provided, of course, the lime is of good quality. When the lime is slacked, add sufficient water to bring the whole up to 25 gallons. When the copper sulphate is entirely dissolved and the lime is cool, pour the lime milk and the copper sulphate solution slowly together into a barrel holding 50 gallons. The milk of lime should be thoroughly stirred before pouring. The method described ensures good mixing, but to complete this work the barrel of liquid should receive a final stirring, for at least three minutes, with a broad wooden paddle.

'It is now necessary to determine whether the mixture is perfect—that is, if it will be safe to apply it to tender foliage. To accomplish this two simple tests may be used. First insert the blade of a penknife in the mixture, allowing it to remain for at least a minute. If metallic copper forms on the blade, or, in other words, if the polished surface of the steel assumes the colour of copper plate, the mixture is unsafe, and more lime must be added. If, on the other hand, the blade of the knife remains unchanged, it is safe to conclude that the mixture is as perfect as can be made. As an additional test, however, some of the mixture may be poured into an old plate or saucer, and while held between the eyes and the light, the breath should be gently blown upon the liquid for at least half a minute. If the mixture is properly made, a thin pellicle, looking like oil on water, will begin to form on the surface of the liquid. If no pellicle forms more lime must be added.'

Iron vessels must not be used for any solutions containing copper sulphate. If used in a sprayer the milk of lime should be strained before mixing. The knife test is not very sensitive, and is usually replaced by the following. Put some of the mixture in a saucer, and add a drop of a ten per cent. solution of yellow prussiate of potash (potassium ferro-cyanide); if the drop changes to a reddish brown colour, more milk of lime must be added.

The quantities given in the foregoing recipe are American gallons, six of which are equal to five English gallons. In English measure the quantities are 42 and 21 gallons approximately, but this correction is practically never made, the weaker mixture with 50 English gallons being used. For spraying cacao at Gangaruwa 45 gallons water, 5 lbs. copper sulphate, and 5 lbs. lime have been used.

Pickering (Eighth Report, Woburn Fruit Farm) has recently advised the use of the following mixture. Dissolve 6 lbs. 6½ ozs. of copper sulphate in two or three gallons of water. Slake some good quicklime with a little water and put it into a tub with 120 gallons of soft water; the amount of quicklime is immaterial as long as it is not less than two or three lbs. Stir up the lime and water two or three times and leave it to settle. Run off 86 gallons of the *clear lime water* and mix it with the copper sulphate solution, and make it up to 100 gallons by adding 11 or 12 gallons of soft water. The same tests should be applied to this as to Bordeaux mixture. It has the advantage of (*a*) cheapness, as less copper sulphate is required; and (*b*) ease of preparation, because lime water is more easily prepared than the milk of lime used in Bordeaux mixture, and does not involve subsequent straining. This mixture is extensively employed in Italy.

Corticium salmonicolor, B. & Br.—Membranous, frequently surrounding stems and branches for a considerable distance, rose pink or ochraceous, loosely adhering to the matrix; minutely pulverulent, finally cracked and areolated: basidia clavate, tetrasporous: sterigmata slender, 4–6 μ long: spores piriform, hyaline, apiculate, 9–12 × 6–7 μ (germinating readily in water).

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A New Stem Canker (*Coniothyrium sp.*).

A disease which causes a true 'canker' on the stem was discovered on a Ceylon estate in 1909, and since then a few more specimens have been received from other localities. It apparently begins always on young green shoots, though its effect is not noticed until secondary growth of the cortex has begun, and often not until the stem or branch is three years old. Probably it may be more common than the recorded occurrences would lead us to expect, having been overlooked because it does not kill the stem, at least not in the time it has been under observation.

The first sign of the disease is the appearance of a hard yellowish patch, due to the development of a corky layer beneath the epidermis, on the green shoot. This patch is confined to one side of the shoot over about one-quarter of its circumference, and as a rule runs along it for about an inch, but it may in some cases extend for a greater length, sometimes for several feet. This appearance cannot, however, be said to be characteristic of this disease, since green shoots often acquire hard patches which leave no trace when the stem has become woody.

The epidermis of the shoot turns grey or almost white, and subsequently splits longitudinally. It is then seen that the underlying cortex had evidently split some time previously, since under the dead epidermis there is a wound bordered by callus apparently healed up quite normally. This stage is shown on Plate 6, A, in which the line down the middle indicates where the calli from the two sides of the wound have met. To all appearance the branch has recovered from the disease; the dead epidermis is being cast off, and the wound in the stem is healed.

But further examples prove that this consummation has not been attained. As the stem increases in thickness the calli on the two sides separate and expose the wood. Fresh calli are produced as the tree endeavours to heal the wound, but these never succeed in meeting. Consequently the wound gradually becomes larger and bordered with irregular gnarled swellings, and at the same time it usually increases in length. Plate 6, B, shows the main stem of a two-year-old tree down which the wound extended for a length of about six feet; it was probably not so long originally, but it has lengthened as the stem grew older. The wood is exposed and partly decayed, and usually blacked by the growth of saprophytic fungi. At first sight it seems as though some animal had torn a long strip of cortex off the stem, and the wound was now healing properly. Only by a long sequence of examples has it been possible to prove that idea incorrect.

An older stage is shown on Plate 6, C. This specimen

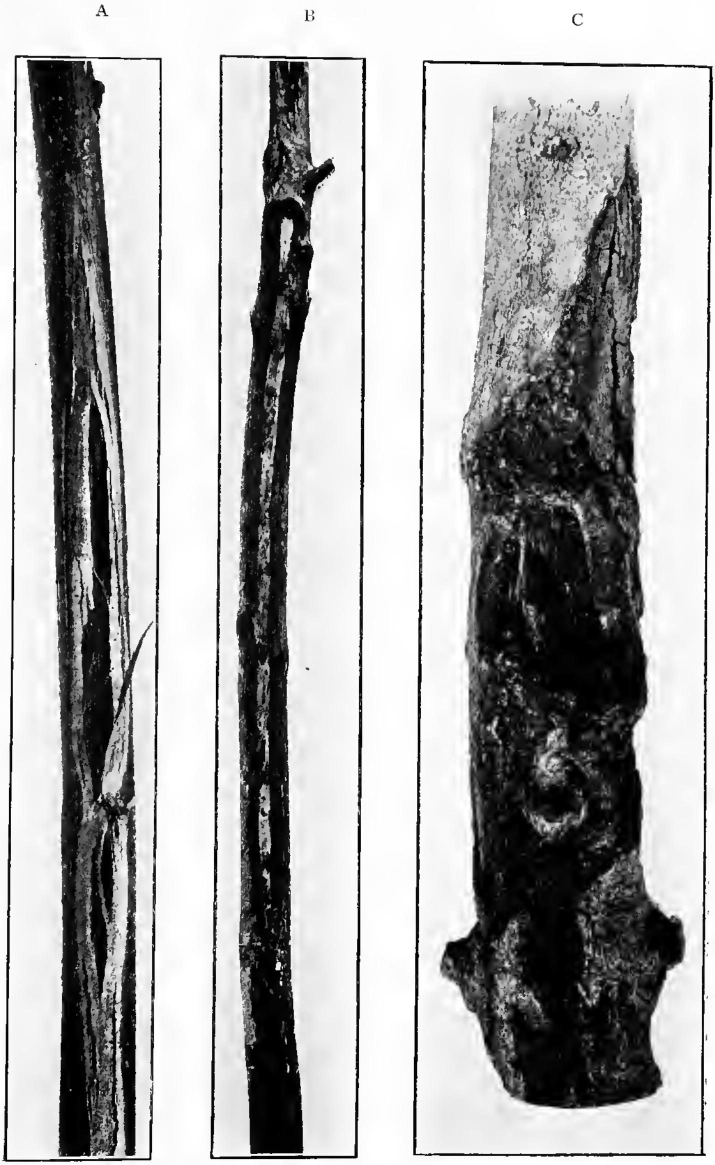

Plate VI.—*Hevea* CANKER.

Caused by *Coniothyrium sp.*

was cut from a large lateral branch on a fourteen-year-old *Hevea*. It is now a true canker—the wood exposed in the centre, surrounded by irregular gnarled calli of different ages.

In the first stages of this disease the dead epidermis contains minute black points, which are the fructifications of a *Coniothyrium*. As the effect is similar to those which have been attributed to various species of *Coniothyrium* on other plants, it is probable that that fungus is the cause of the disease.

Apparently the branches or stems are not killed by this disease, but the exposed wood may be attacked by other fungi which would in course of time hollow them out and cause them to break off. When young stems are attacked they should be cut down below the wound, in order to obtain another sound stem; the wound does not heal—though it is apparently doing so—and the stem will not be of much use for tapping. When lateral branches are attacked they are usually not observed until the wounds are several years old. It is scarcely worth while to cut off such branches, or to hollow them out in an attempt to get rid of the canker. If the exposed wood is tarred, the branch will survive probably as long as the whole tree: but this will not prevent infection of other trees by the spores of the fungus which develop from fructifications in the callus surrounding the wound. If there are no young trees, *i.e.*, less than one year old, in the immediate neighbourhood, such infection will not entail very serious results, since it is only on young stems that the wound is in the way of tapping. The possibility of infection might be diminished by painting the cankers with the sediment from Bordeaux mixture.

Dieback.

In 1905 a disease which attacked the leading shoots of *Hevea brasiliensis* was brought to notice in Ceylon. It occurred especially on young trees one to two years old; instances of its occurrence on older trees were known, but such were rare. It would, however, be difficult to make an accurate diagnosis, offhand, in the case of old trees, since the top shoots may die back from many other causes, some of which will be referred to subsequently. Its earlier stages are seldom noticed on old trees, though they generally attract attention on young trees which have not yet produced lateral branches.

The fungus which begins the disease attacks the leading green shoot. The place attacked becomes dark brown, and this discoloration gradually extends over the whole shoot, while the leaves fall off as the fungus reaches them. The brown patch is frequently rather soft, but it hardens up afterwards and turns grey. It seems to be a universal rule that this fungus does not attack the shoot at the apex, but at the middle of its length, and the brown discoloration then extends upwards and downwards. The colours assumed by the dying shoots are not distinctive; any dead *Hevea* shoot, whether killed by wind or disease, will turn gray; but the progress of the discoloration from the middle of the shoot appears to be peculiar to the true fungus 'dieback.' It will readily be understood that these symptoms are more generally observed on young trees than on old trees; when the former are attacked, the specimens sent in for examination usually include some in the earliest stages, but when old trees are concerned, the specimens have usually advanced far beyond the stage in which the leading shoot only is involved, and therefore secondary symptoms have to be relied on in diagnosing the disease: and the difficulty is increased by the fact that both the fungi found in 'dieback' can grow on dead *Hevea* material, no matter what the cause of death.

The fungus which originates the disease produces its fructification immediately beneath the epidermis on the dead shoot. The epidermis is slightly raised in very minute swellings which afterwards burst at the top and liberate the spores, leaving minute holes which make the shoot rough. If the diseased shoot is kept moist in a glass dish, the spores are seen to be pushed out from the invisible fructifications in thin tendrils which may be either pink or white. This fungus is a *Glœosporium*, and has been named *Glœosporium alborubrum* Petch. It is quite a microscopic species, and has no features which could be noticed in the field, such as *Fomes semitostus* and *Corticium salmonicolor* have.

The green shoots of *Hevea* may die back from many other causes. Wind kills them in many cases, and several instances have been known in which the terminal shoots have died after severe tapping. What constitutes severe tapping must depend upon the age of the tree and its situation. It is impossible to lay down any rules on the subject, and it must be left to the judgment of the planter and his knowledge of the tree to decide, when a tree appears unhealthy, whether its condition may not be due to a too complete removal of the cortex. If this cause is probable the tree should be rested. But this reason for general unhealthiness should be reached only by a process of exclusion, *i.e.*, he should first make quite certain that there is no root disease and that the tree is not ringed by canker. A general defoliation and death of terminal shoots all over the tree is not 'dieback.' Root disease should always be suspected in such cases, and the earth should be scraped away round the stem to see whether any fungus is present or whether the roots show signs of decay. In prolonged wet weather a general defoliation with some dying back of green shoots may occur as a result of the climatic conditions; this has already been referred to.

When green shoots are killed by wind they usually die back from the tip: this distinguishes them from shoots killed by *Glœosporium*, if they are seen in the early stages. I have a record of one case, at a fairly high elevation, where the leading shoots of young trees are periodically killed off during the south-west monsoon.

When *Hevea* fruits have been attacked by *Phytophthora* the fruit-bearing shoots frequently die. This is, no doubt, due to the *Phytophthora* working back from the fruit to the stem, as it does in the case of cacao, though the fungus has not yet been isolated from such branches. No case is known in which the *Phytophthora* has travelled from the fruit to the main stem of the tree *viâ* the green shoot.

It is, I hope, clear from the foregoing that green shoots may die back from several causes, and that one of these causes is the attack of a fungus, *Glœosporium alborubrum*. This has been known in Ceylon since 1905, but no serious damage has ever resulted from the death of *green shoots only*. The trees send up fresh shoots from buds lower down the stem, and very little harm is done.

There may, however, be further developments which are much more injurious. A few cases occurred on young trees from time to time in Ceylon, but in 1909 it assumed a more serious aspect, trees from nine to fourteen years old being killed with astonishing rapidity on several estates. These trees occurred in groups as a rule; and while some of them were dead almost to the base, others were in the first stages. This further death of the woody stem is due to the attack of another fungus, *Botryodiplodia theobromæ*, Pat., and in the cases examined it followed the previous death of the green shoot from the attack of *Glœosporium*. After the death of the leading shoot, *Botryodiplodia theobromæ*, which lives on dead *Hevea* and can attack living stems through wounds, entered the dead shoot and grew downwards in the woody stem, gradually killing it down to the base.

To clear up misunderstandings which have arisen from previous accounts of this disease, the position may be summarised as follows. Green shoots of *Hevea* may die from several causes, one of which is *Glœosporium alborubrum*. The death of such shoots may be followed by the death of the whole tree from the attacks of *Botryodiplodia theobromæ*. In the cases examined, *Botryodiplodia* has entered the tree after the shoots have been killed by the *Glœosporium*, not after they have been killed by wind or climatic conditions. As was previously stated, this further development is not universal, and when it does not happen, 'dieback' is negligible. Moreover, it is quite possible that the *Botryodiplodia* may enter shoots which have been killed in some other way; but this is an obvious suggestion which awaits proof. It has been stated that cases of dieback are not all due to the same cause; whether this is true or not depends, of course, upon the knowledge of the individual who diagnoses the case.

Botryodiplodia theobromæ has long been known as a parasite of cacao, and it has been proved by Howard, and Van Hall and Drost, to be only a wound parasite: it does not attack stems or pods unless they have been previously wounded or attacked by other fungi. But even as a wound parasite it is selective. For example, it grows on *Hevea* pods which have been killed by *Phytophthora*, and also on cankered *Hevea* bark, but no case has yet been recorded in which it has attacked the exposed cortex of the tapping cuts.

When *Botryodiplodia theobromæ* has entered a dead shoot it proceeds down the tree, both in the wood and the bark, but chiefly in the former. Both are blackened by the mycelium of the fungus, and the cambium becomes a black film on the surface of the wood. In early stages the cambium is converted into a black or dark brown slimy layer, but this subsequently dries, and the bark may crack and peel off. The fructification is produced in the bark. It is a small black sphere, about one-hundredth of an inch in diameter, filled with spores. If the bark is shaved away carefully these spheres are cut across and appear as black circles with a white centre, the spores appearing white when immature. The spheres frequently occur close together and united into a continuous mass; this happens especially when they develop in cracks in the bark, and in such cases they may form a projecting swollen cushion. The spores are extruded from the spheres when ripe, and cover the surface with a fine black powder like soot. In dry situations this powder may be white at first, but in its fully-developed form it is black. When examined under a microscope the spores are found to be oval, with a transverse wall across the middle; this is the characteristic *Diplodia* spore. In this case also the fungus is microscopic, its spores, by which it is identified, being only about one-thousandth of an inch in length and half that in breadth. It cannot, therefore, be readily identified by the planter, though if he possesses a microscope he can soon become acquainted with its spores.

The progress of the disease on trees with well-developed branches is very characteristic. As the fungus grows down the stem it kills off the whorls of branches in succession, the part below meanwhile remaining quite healthy and in full foliage. Thus, if a tree has four whorls of branches, the death of the leading shoot is followed after a short interval by that of the uppermost whorl of branches, the three lower remaining normal. Then the second whorl dies, while the lower two are not affected; and so on until all the branches are killed and the stem is dead down to the base. The progress of the disease appears to be very rapid; most of my correspondents state that the tree is dead within a month or six weeks after the death of the uppermost branches. The trees are usually attacked in groups, sometimes of about a dozen; one or two of these are usually killed, but the others are generally in earlier stages and can be saved by pruning off the dead tops.

Practically the only other stem disease at present known, whose effect could be confused with that of 'dieback,' is the 'pink disease' caused by *Corticium salmonicolor*. In this case the whole of the branches above the point attacked may die off, leaving the lower branches still healthy; but this disease is easily distinguished by the pink sheet of fungus tissue on the stem, usually where several branches arise close together.

When trees are attacked by 'dieback' which extends to the woody stem, their tops must be cut off below the dead part and burnt. If the diseased stems are left lying about the plantation they will hatch out myriads of *Botryodiplodia* spores, all ready to attack other trees when they get an opportunity. *Botryodiplodia theobromæ* is an extremely widespread fungus, and it would be quite impossible to eradicate it, but there is no need to encourage it by neglecting to remove the dead stems. It grows excellently on all dead stems of *Hevea*, no matter what the cause of death, and any one can obtain specimens of the fungus by cutting down a healthy tree and leaving the stem on his verandah for a fortnight. It is fortunate that the fungus is not a direct parasite of *Hevea*, but can only attack it through wounds or dead branches.

The diseased part should be cut off with a slightly sloping cut. If the dead portion is large it should be cut down in sections so as not to injure the lower branches. The coolie will, no doubt, prefer to use a catty, but if so, the stem should be finally trimmed with a saw to get a smooth surface. The cut surface must be tarred to prevent the entrance of fungi.

Whether it is worth while to remove all dead green shoots is open to question. Probably in the majority of cases their death does not lead to anything further. But it would be a precautionary measure which might obviate future loss through the attacks of *Botryodiplodia*.

Dieback of *Hevea*, as a result of the attack of *Botryodiplodia*, is known to occur in Ceylon, South India, and the Federated Malay States. In the latter country it was said to be likely to prove as dangerous as *Fomes*, if not worse; one instance was recorded in which a two and a half year old tree was killed back to four inches from the ground within twelve days. Some confusion has been caused in this case by the identification of the fungus, first as a *Cucurbitaria*, and afterwards as a new species of *Diplodia*, viz., *Diplodia rapax*. There is no ground for the suggestion that it is a stage in the life history of a *Rosellinia*.

Botryodiplodia theobromæ is an extremely widely distributed fungus, and has been recorded for almost every country in the tropics. But as the original description was incomplete and the fungus is very variable, it has received an extraordinarily large number of different names. To a great extent this confusion has been brought about by the fact that the fungus has usually been described in temperate countries from specimens sent from the tropics, and these specimens have been insufficient to permit any idea of its variability to be obtained. Among the names which are known to refer to this species are *Macrophoma vestita*, *Diplodia cacaoicola*, *Lasiodiplodia nigra*, *Botryodiplodia elasticæ*, *Chætodiplodia grisea*, *Lasiodiplodia theobromæ*, *Diplodia rapax*; and there are probably many others. *Botryodiplodia theobromæ* is its earliest name, as far as is known at present; but some prefer to call it *Lasiodiplodia theobromæ*.

This fungus is known to grow on cacao (stems, roots, and pods), sugar cane, *Albizzia moluccana* (roots), papaw (stems), mango (fruits), and *Castilloa* (stems). In Ceylon it has been found (1) on *Hevea brasiliensis*, killing back the main stem or causing the death of 'stumps,' or living as a saprophyte on dead *Hevea* stems and pods; (2) on *Cacao* as a wound parasite which kills back the twigs, as a saprophyte on diseased cacao pods, or causing a dry canker on large branches; (3) on *Castilloa* stems which have been previously injured by fire; (4) on dead papaw stems, in which case it is merely saprophytic; (5) on wounds on the stems of old dadaps, and on decaying dadap logs; (6) as a saprophyte on dead stems of *Ficus elastica* which were healthy when cut; (7) on pruned stems of *Albizzia moluccana*, which it entered through the cut surface and killed down to the base; (8) on tea, which it enters through the roots and kills; (9) on the roots of coconut palms killed by *Fomes lucidus*.

The above is a formidable list of plants which can serve as hosts for *Botryodiplodia theobromæ*. Yet in spite of that it has never caused serious damage in Ceylon. It is widely distributed, but in the majority of cases it is only saprophytic, though it does sometimes become a wound parasite. If dead branches are regularly removed and burnt, both in cacao and *Hevea* plantations, it is not likely to cause much loss.

Glœosporium alborubrum, Petch.—Pustules 150-200 μ diameter, black, rupturing the epidermis irregularly; spores hyaline, oblong, ends rounded, straight or slightly curved, issuing in thick, pink or white tendrils, 15–20 × 3-4 μ.

Botryodiplodia theobromæ, Pat.—Pycnidia scattered or confluent, sometimes united into a stroma which bursts through the cortex in linear or rounded masses 1·5–2 mm. diameter, smooth or clothed with loose hyphæ; pycnidia 0·25–0·4 mm. diameter; spores 25–35 × 14–15 μ, oval, one-septate, fuliginous or blackish brown; paraphyses abundant, linear 40–80 μ long.

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FUSICLADIUM SP.

A stem disease attributed to a species of *Fusicladium* was discovered in Java in 1907 by C. Bernard and described by him as follows:—'I have on several occasions received for examination material attacked by a black canker of the stem, caused by a fungus belonging to the genus *Fusicladium;* I have not been able to determine the species. The disease is not yet serious; in two different plantations it had attacked a group of a dozen trees. A little later, in one of these plantations the disease attacked a new group of about forty trees, of which thirty died. This spot was about half an hour distant from the first. The disease begins on pruned branches and on the top of stems which have been pollarded; the leaves wither, turn yellow, dry, and then fall, the flow of latex diminishes rapidly and soon ceases altogether, and after a few days the tree is dead; sometimes only the upper parts die, and new shoots are produced below the part attacked; the bark of the diseased part cracks, and scales off, and between it and the wood, in addition to a number of insects whose occurrence is only secondary, is found the fungus which causes the disease, its mycelium forming a blackish down on the surface of the wood. With the aid of the microscope, brown, uniseptate conidia, characteristic of the genus *Fusicladium* are easily found among the mycelium. The hyphæ are brown, septate, and branched; after they have destroyed the soft parts of the cortex and the bast, they penetrate into the young wood and give it a dark colour.

The disease is contagious, since it spreads to neighbouring trees; the conidia enter the stems through accidental wounds or, as already stated, through the wounds resulting from pruning or pollarding; and the mycelium proceeds from the top of the stem to the base.'

The treatment for this disease is the same as that recommended in the case of *Botryodiplodia theobromæ* (p. 220). Indeed, the two diseases so closely resemble one another that one might suggest that they are really identical.

REFERENCE.

Bernard.—*Bull. Dept. Agric. Indes Néerlandaises*, No. xii.

A DISEASE OF 'STUMPS' (*BOTRYODIPLODIA THEOBROMÆ*, PAT).

In several cases *Hevea* stumps which have been planted out under favourable conditions have failed to grow, and in one instance, in Burmah, sixty per cent. died. Generally the plants made no growth whatever, but occasionally some developed a green shoot about six inches in length, and then died. They were usually sent in as examples of the damage done by white ants, because the tap-root as a rule had been eaten away, all the cortex having disappeared, and only an irregular spindle of wood being left. In practically every case it was determined that death was due to *Botryodiplodia theobromæ*, the fungus which causes the chief damage in 'dieback.' When the stem was split open, black streaks were found running longitudinally down the wood; this discoloration was due to the dark-coloured mycelium of the fungus which filled the cells of the wood and made it appear dark wherever the fungus had penetrated. The cortex was also blackened (when present) and contained the characteristic fructifications of the fungus; and in some cases these formed large black cushions at the points of origin of the secondary roots after the latter had disappeared. Beetles were often found in the dead stems, but these had not contributed in any way to the death of the plants.

In these cases the fungus had entered the plant, not at the top as in 'dieback,' but either below ground or at the collar. It is probable that in the majority of cases its entrance was facilitated by injuries inflicted during planting; but in one case at least it would appear that the fungus was able to attack uninjured plants.

In general, further loss was avoided by liming the holes where the plants had died, and supplying with basket plants. But in one case some of these supplies also died from the same cause; it is this case which lends colour to the supposition that *Botryodiplodia theobromæ* may be able to live in the soil and attack the roots of uninjured plants, as is a similar *Diplodia* which attacks maize in America.

The disease has proved worse on old chena land than on land which was virgin jungle. It was recorded from Ceylon and Burmah in 1906 and from the Federated Malay States in 1909. In the latter instance, the stumps attacked were about three inches in girth, and eighty per cent. were killed. Those killed in Ceylon were, as a rule, about two inches in girth.

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A Stem Disease of Seedlings (*Pestalozzia palmarum*, Cooke).

Pestalozzia palmarum is the fungus which causes the well-known grey blight of tea, and the common leaf disease of coconuts and other palms. It is often found on diseased leaves of young *Hevea* in the nursery, but as it is much more vigorous as a saprophyte than as a parasite, there is usually much doubt whether it is the cause of the injury.

In several instances it has been found to attack the green stems of seedlings about a foot high. The stems were attacked at the collar, and killed by the fungus to a height of about an inch. The death of this part was of course followed by the death of the whole plant, because the supply of water was cut off. The diseased part of the stem was white externally, and separated from the green part above by a narrow red-brown line. Seeing that *Pestalozzia* is in many cases saprophytic, it must be considered probable that the general condition of the nursery may have contributed to this result.

When diseases of this kind make their appearance in the nursery, all the plants attacked should be rooted out, and the affected patches, *i.e.*, the bare soil, watered with a solution of carbolic acid or Jeyes' fluid, one ounce to a gallon of water. The shade should be removed as much as possible, at least at intervals.

In many cases the general condition of the nursery, not any specific disease, is responsible for the death of seedlings. Nurseries are not a conspicuous success, as a rule, although the majority of the plants manage to survive. The preparation of seed-beds and the care of nurseries is more a gardening operation than the usual routine of a plantation, and in many cases it does not receive sufficient attention. The site of a nursery is usually conditioned by the water supply, and the same ground is often used for nurseries continuously; consequently it becomes sour and quite unfit for the purpose. The position of the nursery should be changed, and the old ground allowed to lie fallow, and dug over repeatedly before being used again. If any disease has occurred the ground should be sterilised, either by surface firing or by the use of formalin. Surface firing is done either by burning straw, &c., over the bed for about an hour, or by removing the surface soil to a depth of about six inches, heating it in an open pan, and then replacing it.

In the formalin treatment the soil is thoroughly pulverised and then drenched with a formalin solution composed of one part of commercial formalin to 150 or 200 parts of water, from three-quarters to one gallon being used for each square foot. The solution is applied with a watering-can fitted with a rose, and distributed as evenly as possible, so as to thoroughly wet the soil to a depth of a foot. In most cases it is necessary to put this solution on in two or three applications, as the soil will not take this quantity of liquid immediately. The beds are then covered with sacks or tarpaulin to keep in the fumes for a day or so, and then aired for a week before sowing the seed. In some cases a lower percentage germination has been recorded after the formalin treatment, but a greater number of plants survived than in the untreated bed.

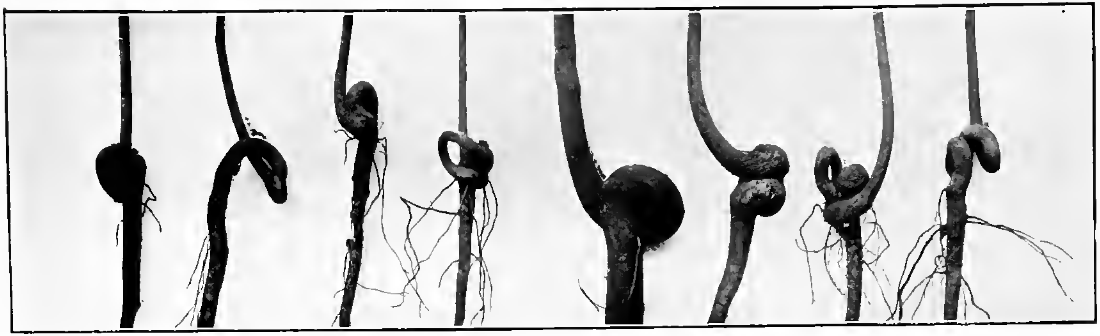
Sterilisation by steam is now extensively used in the United States, but the amount of loss sustained in estate nurseries in the East scarcely justifies the adoption of such an expensive method. The most effective steam treatment is the inverted pan method, in which a galvanised iron pan, six by ten feet, and six inches deep, is inverted over the soil and the steam admitted under pressure; the pan has sharp edges which are forced into the soil to prevent the escape of steam.

CHAPTER XII.

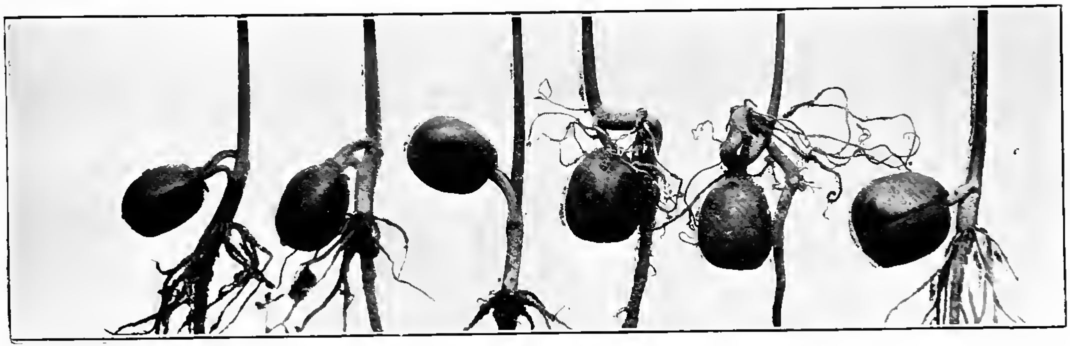
ABNORMALITIES IN *HEVEA*.—TWISTED SEEDLINGS.

CONSIGNMENTS of nursery plants with twisted stems have frequently been sent in for examination and report. In the most general case the stem makes a complete turn at the base, either in a regular curve or a combination of curves and abruptly angular bends; in other cases there are two complete turns, and in a single instance three have been observed. The curves are rarely all in the same direction, and the resulting combination is frequently so complicated that analysis fails to produce any adequate explanation. It may be remarked that the examples given here have been grown in free soil, and do not owe their origin to the presence of stones in the nursery. As a rule, specimens are discovered when the plants are twelve or eighteen months old, *i.e.*, when they are about to be planted out in the field; at that time the coils are generally fused together, and if allowed to grow would ultimately form a solid mass at the base of the stem. These twisted plants are usually discarded (in theory at least); but one planter informs me that if the tap-root is removed they are admirably adapted for planting in swampy land, where a long tap-root will not grow well; in that case the mass of roots which spring from the knot provides a better growth than a sickly tap-root would.

The specimens photographed (Plate 7A) are about eighteen months old. Taking them in order from the left, No. 1 shows a curve over to the front, followed by a curve to the left and a complete turn; it is really two complete turns, with a reversal of direction half-way through the first. No. 2 is a single turn, abruptly bent into another plane midway. No. 3 is also a single turn, with a similar abrupt bend, but in this case the bend is outwards, and therefore the loop is not completed. No. 4 shows a complete turn over to the back, followed by a complete turn over to the left, the end of the second loop coming up behind the first loop; but although this appears the most complicated of the first four, it is really



(*a*) Eighteen months old. $\times \frac{1}{6}$.



(*b*) One month old. $\times \frac{1}{2}$.

Plate VII.—Twisted Seedlings.

the one most easily explained. No. 5 is a single complete loop, without abrupt bends, and probably for that reason this specimen has attained a greater size than the others. No. 6 has a complete turn to the right, followed by another to the left. No. 7 has a half-turn forward, fused into a solid knob, a complete curve to the back, and a half-turn upwards; it is identical in origin with No. 4. No. 8 shows two complete turns, the first being reversed half-way through; it is the same as No. 1.

During an investigation into the germinative capacity of various samples of seeds in 1907, the genesis of these abnormalities was observed in several instances, and the results of these observations were confirmed by further experiments in 1908. In these cases the seeds were planted in seed pans in potting mould, which was sifted into the pans, and the pans were watered daily; the results cannot be attributed to stones or to caking of the soil.

A knowledge of the structure and germination of the seed is essential to the correct understanding of the origin of these abnormalities.

The seed is a slightly flattened ellipsoid, rather larger at one end. One of the broad faces is usually rounded; this may conveniently be called the upper side; it is the outer side when the seed is in its natural position in the capsule. The lower face is grooved down the middle, and has usually two flattened areas, one on either side of the groove, showing where it was in contact with the partitions of the capsule. Along the groove lies an adherent strand known as the raphe; it terminates at the broader end of the seed in a depressed area called the chalaza, which is usually indicated by three converging colour bands. At the other end of the seed is the micropyle, easily recognised by its circular operculum, 3–5 mm. in diameter, which fits closely into a corresponding aperture in the shelly seed wall, and is held in position by the thin outer coat. There is usually a minute point in the centre of the operculum. The hilum is marked by a slight depression on the lower surface a little below the micropyle.

The seed varies enormously in size and weight. M. Vernet (Annam) records weights of 1·02 grammes and 9·55 grammes; he says also that the seed is the size of a chestnut. The Annam trees are grown from Ceylon seed, but the Ceylon seed does not show such wide variation, and is only about half the size of a chestnut.

Frequently the seed bears a rough excrescence, usually near the top. This varies considerably in size, and may cover the whole of one side of the seed; it is usually whitish, and lacks the polished, mottled covering of the seed proper. In the cases examined this excrescence is hollow, and the normal shelly coat is not developed beneath it. This outgrowth has been styled an aril or caruncle by some authors, but it appears to be rather an abnormality, depending for its origin on the wall of the capsule. Sometimes it is fused with the wall of the capsule. It occurs on about one seed in a thousand at Peradeniya.

The embyro (*i.e.*, the young plant within the seed) consists of two large flat cotyledons (seed leaves), lying parallel to the upper and lower surfaces of the seed, with a minute radicle (root) and plumule (shoot) at the micropylar end lying straight along the axis of the seed. In a cross section the cotyledons appear as a narrow band extending almost completely across the middle of the seed, surrounded by the endosperm; the latter is a mass of tissue containing food for the young plant in the form of oil and starch. The arrangement of the cotyledons, however, is variable, and the band seen in the cross section may be wavy, or V-shaped, or triangular when one of the cotyledons is folded down the middle, or X-shaped when the cotyledons separate at their outer edges. The cotyledons lie close together when the seed is fresh, but separate and leave a large central hollow as the seed dries.

The seed germinates in about ten days when fresh. The radicle pushes off the operculum and emerges as a white stump about 3 or 4 mm. in diameter, with a truncate flattened end. As it lengthens the flattened end becomes slightly conical, and its margin develops a number (usually 12) of minute points. The conical central point is the developing tap-root, while the marginal points are the developing (secondary) lateral roots (Plate 8, fig. 1). It is a peculiar feature of *Hevea* that the secondary roots at first grow much more rapidly than the primary root, and serve to fix the young plant as the radicle curves downwards (Plate 8, fig. 2). Strictly speaking, the curved structure in fig. 2 is not the root, but the hypocotyl, *i.e.*, the transitional region from root to stem, which lies between the top of the root and the bases of the two seed leaves or cotyledons.

The cotyledons remain inside the seed, absorbing food

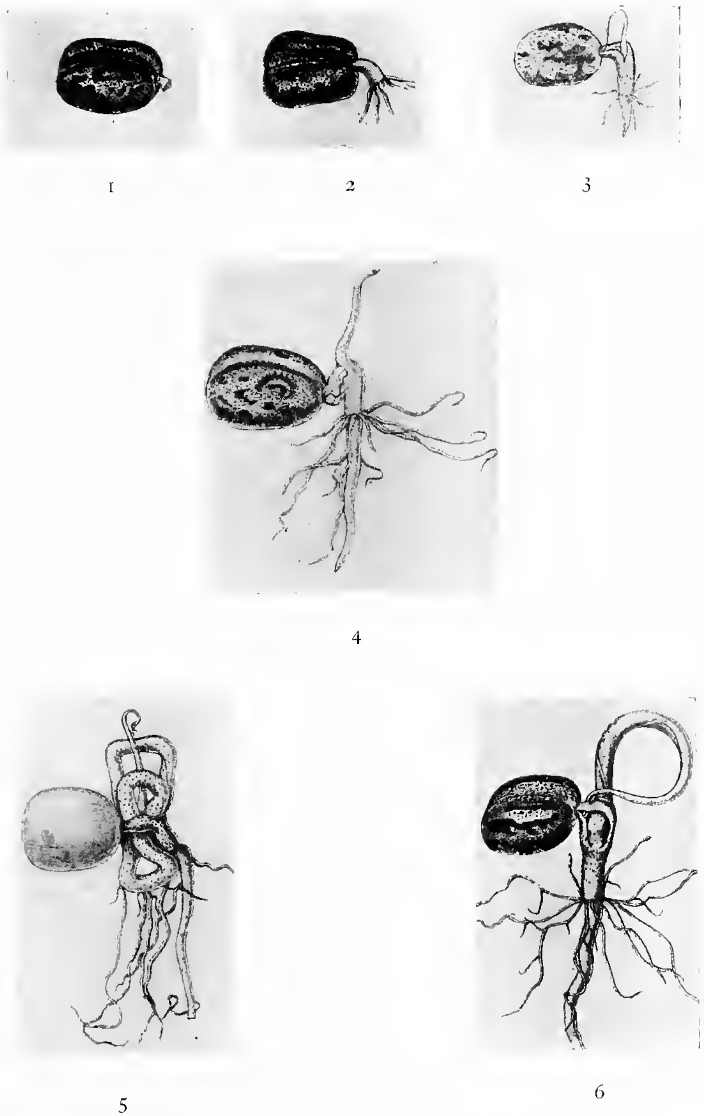


Plate VIII.—Germination of *Hevea*.

materials from the endosperm, and forwarding them to the growing points of the young plant. But the stalks of these leaves are lengthened until they project outside the seed for a distance of about a centimetre. They are shown in figs. 3–6 joining the young plant to the seed. At first they lie in contact with one another, and if the seed is lying horizontally, *i.e.*, either on its upper or lower surface, the slit between the leaf stalks is also horizontal. The slit is present in the specimen of fig. 2, but it is not visible in the position in which the seed has been drawn. It is important to grasp the position of these leaf stalks, because most of the abnormalities here described are caused by them.

In the stage represented in fig. 2 the young shoot lies between the two leaf stalks, and it maintains this position as the leaf stalks elongate, its growth keeping pace with them, so that its tip is always situated just within the seed. By the time the elongation of the leaf stalks is complete, the tap-root has developed and the root system is prepared to supply as much water as is required. The shoot (plumule) now grows vigorously, emerging *sideways* from between the leaf stalks in a loop, which soon turns upwards as in fig. 3. The further growth of this loop pulls the tip out of the seed, and the whole shoot becomes vertical (fig. 4). If germination has been normal, the leaf stalks are usually twisted out of their horizontal position during the extension of the shoot.

After a number of chance observations on the formation of twisted stems the following experiments were carried out:—

(*a*) 50 seeds were planted horizontally with the lower surface downwards. (This is considered the proper position by most planters.)

(*b*) 50 seeds were planted horizontally with the lower surface uppermost.

(*c*) 50 seeds were planted vertically with the micropyle downwards.

(*d*) 50 seeds were planted vertically with the micropyle uppermost.

(*e*) 50 seeds were planted horizontally and on their narrower sides.

The results are illustrated in Plate 7 B. No. 1 represents the seedlings of lot (*a*), in which 48 germinated, all normally. No. 2 represents the seedlings of lot (*b*), of which 49 germinated, all normally. No. 3 is a seedling of lot (*c*), of which 47 germinated; the seeds were raised above the soil, but the plants were normal. Nos. 4 and 5 illustrate the seedlings of lot (*d*), in which 45 germinated; 27 had a knee bend as shown in No. 4, and in 9 others this was accentuated and became N-shaped, while the remaining 9 possessed complete loops as illustrated in No. 5. No. 6 is a representative seedling from lot (*e*), in which 39 germinated, all normally.

Experiment (*d*) was repeated with 20 seeds; 15 of these germinated, 11 with simple knee or N-shaped bends, and 4 with complete loops. It seems therefore established that planting the seed vertically with the micropylar end uppermost favours the production of twisted seedlings.

When the seed is planted in this position the radicle emerges vertically and bends over usually towards the raphe, *i.e.*, the 'lower' side of the seed; and unless the seed is planted deeper than usual, the radicle comes above ground before it begins to curve. The secondary roots are then produced in the air, and may wither before reaching the soil. This is the extreme case; but in all cases even when the radicle does not emerge from the soil, the emergence of the looped shoot is considerably delayed, because it does not occur until the radicle has curved over so that its tip points vertically downwards and the secondary roots have been produced. During this delay all the parts outside the seed, *i.e.*, the hypocotyl and the two leaf (cotyledon) stalks, become much thicker than in normal germination. (They also show a greater development of the usual warts—lenticels—on the hypocotyl.) This thickening has two results; it makes the curve in the hypocotyl and the base of the shoot so rigid that they cannot subsequently straighten, while the two leaf bases hold the shoot, when it endeavours to straighten out, as in a vice. These two factors produce all the twists.

Since the slit between the leaf stalks is horizontal, the loop of the plumule (shoot) must emerge from it horizontally and at right angles to the root. It then turns so that its two sides are in the same vertical plane. The tip is then pulled out of the seed, the loop straightens out, and the whole shoot curves upwards and into the same vertical plane with the root. There is then formed a knee or an N-bend, according to the amount of curvature in the seedling before the plumule straightened (see No. 4, Plate 7 B; the position of the cotyledonary petioles is due to subsequent torsions).

Nos. 2 and 3 of Plate 7 A, are also simple N-bends; in 3 the plumule bends up outside the curve of the hypocotyl, while in 2 it crosses that curve.

But in many cases the loop is held so firmly by the thickened leaf stalks that it cannot pull out. No. 5, Plate 7 B, shows this; the loop has not straightened out, but the tip of the shoot has emerged from the seed, and has curved upwards on the wrong side of the cotyledon stalks, forming a complete loop. Nos. 4 and 7 of Plate 7 A, are much better examples; the whole loop here is the loop of the plumule, which was held in its original position by the cotyledon stalks; the curve from the root is the original curve in the hypocotyl, and the final curve upwards is, of course, due to the usual curvature (negative geotropism) of the shoot. A further complication can occur during the upward curve of the shoot. For when the tip emerges from the seed, and the loop remains fixed in position by the cotyledonary leaf stalks, the tip is never quite horizontal; usually it is already curved slightly upwards, and continues to grow in that direction; but it may be curved downwards, and in that case, in its subsequent growth, it curves down and round towards the seed, crossing both branches of the fixed loop as it turns up again; there is then formed an 8-shaped figure.

No. 5 of Plate 7 A, is a simple curve, probably formed in the same way as No. 5 of Plate 7 B. Nos. 1, 6 and 8 of Plate 7 A, show a complication which apparently cannot be explained in this way; the first curve from the root is the bend in the hypocotyl, and the succeeding upward turn is the upward of the plumule; so far they are simple N-bends; but it is not clear why the plumule should have made a second complete turn. They are probably caused by the original loop in the plumule being fixed by the cotyledon bases, long enough for the older (proximal) half of the loop to become rigid, after which the tip was pulled out on the proper side of the seed and curved upwards as usual. If this is correct, the older (proximal) half of the original loop lay uppermost in No. 8 and undermost in No. 1. But this condition has not been reproduced in pot experiments.

Another cause of loops is not dependent upon the position of the seed. No. 6 of Plate 8, which was drawn from a pot seedling, illustrates this. The plumule in this case makes a complete loop—which is the original loop as in No. 3—and its tip is seen emerging on the wrong side of the slit between the cotyledon leaf stalks. Thus, unless the upper leaf stalk broke there would be a permanent loop. This again appears to be due to excessive thickness of the leaf stalks, because the loop is held in position by the minute first leaves at the apex of the plumule, which are firmly wedged between the cotyledon leaf stalks in the micropyle. This example furnished an explanation of another anomaly often observed in nurseries. It was watered somewhat excessively, and this caused so great a tension in the loop that it broke off close to the tip. The straightened plumule had then no growing point. 'Headless' seedlings of this description, with a shoot about eight inches long but without a terminal bud, are of frequent occurrence, and this example shows that the decapitation is not necessarily performed by insects. They produce shoots from the two buds in the axils of the cotyledons, and thus double-stemmed plants are formed.

No. 5 of Plate 8 is a complication which I do not attempt to explain. All the curves are in the plumule. The first (from the root) is apparently the original loop. Its tip seems to have been pulled out of the seed normally, but the loop has not straightened out. Instead the tip has turned upwards and grown past the cotyledon bases (it does not go between them, as the figure seems to show). But after attaining an erect position it makes another complete curve, and this third loop passes through the slit between the cotyledon bases just as the first loop does. There does not seem to be any reason for this third curve.

These experiments have been repeated by Stockdale, in British Guiana, with similar results.

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NODULES.

Mention has already been made (Chapter IV.) of the occurrence of excrescences or burrs on the stem of *Hevea,* which may interfere with tapping by ordinary methods; and it has been pointed out that these are of two kinds. In the one case their formation depends entirely on wounds extending to the wood; the wood within the burr is, in such examples, merely a swelling on the wood of the main stem. Such excrescences are, as a rule, small, and since they are

Plate IX.—Burrs on a *Hevea* Stem.

gradually smoothed down by the general growth of the stem they do not offer very serious obstacles to tapping.

The burrs which interfere with tapping are practically always of the second type. They project abruptly from the stem in a more or less rounded outline, so that the tapping cut cannot be continued across them (Plate 9). This is usually the stage in which the planter notices them, but, if they are to be economically removed, search must be made for them when the trees are younger and the burrs are small. They may be found on the trunks of many trees four years old, though fortunately not universally.

The bark is at first slightly elevated, and forms a small hemispherical lump about the size of a pea, or in some cases the swelling is vertically elongated. If the lump is cut open there will be found inside a core of wood, either spherical or cylindric, corresponding to the external shape of the swelling (Plate 10). These cores or nodules are at first symmetrical, without any projecting points, and have no connection whatever with the wood of the stem. They lie wholly in the cortex, separated from the wood and cambium of the stem by ordinary laticiferous tissue. When the surface of the excrescence is cut away, these cores shell out quite easily from the surrounding cortex, separating from it along their cambium layer in the same way as ordinary cortex strips off the wood of a stem. These facts show without any need of microscopic examination that each of the woody cores possesses a cambium of its own, not connected with the cambium of any other knot, and certainly quite distinct from the cambium of the main stem. Each excrescence can therefore increase in size by the addition of new wood to the core and new bark to the outer tissues, quite independently of the growth of the main stem, merely through the activity of its own cambium. A specimen in the Botanic Gardens, Singapore, is 'irregularly hemispherical, or rather half-oval, and measures eighteen inches across transversely, a foot long vertically, and about eight inches in thickness.'

As a rule, several of these small excrescences are produced close together, and, as they increase in size, their cambiums come in contact and the woody cores are fused together. In this way the core becomes irregular. Instances of this fusion are shown in the photograph of Plate 10 A: indeed, not more than five of the cores shown there are simple. If a large number of nodules occur close together then their fusion produces a large thin plate of wood, as shown in Plate 10 B, where the individual cores can still be identified,

As the nodules increase in size the bark frequently cracks, and it is usually not until then that attention is called to it. In the case of rounded nodules, a smaller mass of wood is required to produce cracking than in the case of flat, plate-like nodules, because the latter, though larger, are really younger; the rounded nodule is formed by several years' growth of a single spherical core, as a rule, while the plate only represents a large number of recently formed cores. Such external cracking is, however, not universal.

With increase in size there occurs a change of shape which has led to an erroneous interpretation of the origin of these structures. The inner surface of the woody core produces a conical point, which ultimately fuses with the wood of the main stem. In small cores there is only one projecting point, but in the case of plate-like cores these points may arise all over the inner surface. They are present in the inner surface of the specimen of Plate 10, B, which has the outer bark still attached, but they do not occur in any of the other examples. Theoretically, this fusion should progress until the core is completely fused with the wood of the stem, but I have not yet seen such a case, though some of my specimens of cores are 9 cms. long, 8 cms. wide, and 5 cms. thick. The formation of this point appears to be due to the pressure exerted by the developing core, which apparently prevents the formation of normal cortex between it and the wood of the stem at the points of nearest approach.

In some cases the cortex which covers the burr cracks and dries up; no latex can then be obtained by tapping over the burrs, because the only laticiferous tissue lies behind the woody core. In one particular case a tree twelve years old has not yielded any latex, because its stem is covered with these burrs. In rare instances, on the other hand, the whole of the cortex containing the woody core splits away from the wood of the main stem without any external cracking, and the vacant space is filled by a clot of rubber. I have taken three ounces of almost dry rubber from such a situation. Latex may still be obtained by tapping over the burrs on the tree photographed (Plate 9); these are about two years old. It may be noted, as a curious phenomenon

A

Natural size.

B

One-half natural size.

Plate X.—Nodules extracted from Burrs.

not yet explained, that when latex is obtainable by tapping over these burrs, it often varies in colour from pale yellow to deep chrome.

A cross section through one of the cores shows that its nucleus consists of a small group of dead bark cells (cortical parenchyma), or of the 'stone cells' (sclerenchyma), which occur in *Hevea* bark. The group is vertically elongated in the case of cylindrical cores. A cambium is developed round this nucleus, and this produces short wood cells and fibres arranged horizontally and more or less concentrically. This concentric arrangement is quite clearly seen when the core is cut across. The wood differs altogether from that normally produced in the main stem, and closely resembles 'wound wood.' I have, however, one example of cylindrical cores in which the wood possesses normal vertical vessels and medullary rays arranged in the same direction as those of the stem without any evident relation to the central nucleus.

The structure of these cores is identical with that of the similar bodies found by Sorauer in the cortex of apple and pear trees, and by Krick in the cortex of beech trees. They are usually known as 'Knollenmaser' or 'Rindenknollen.' No reason is at present known for their formation. In the case of the beech, they are sufficiently common to be regarded as a normal feature of the tree. The burrs of *Hevea* agree with all these in showing no trace of insect or fungus injury, and in occurring on surfaces which have not been wounded. Their structure and position in the cortex entirely contradict the theory that they are caused by 'dormant buds'; indeed, the latter idea has no botanical foundation whatever.

During the last five years I have repeatedly advised correspondents to cut out the cores of these burrs before they attain any considerable size. There is no need, then, to make a large wound. If the outside of the burr is sliced off, the core can be shelled out quite easily. But if they are allowed to grow, they become united at several points to the main wood, and a very ugly wound results when an attempt is made to cut them out. It is very doubtful whether it is worth while to try to cut out large burrs which are united to the main stem, because the injury to the main wood will inevitably prevent the formation of a smooth renewed bark. The objection has been made that shelling out the cores of small burrs exposes the cambium, and this cambium dies. So it does; but it is the cambium of the burr, not the cambium of the main stem. It must be remembered that where these burrs occur there are two cambiums, at first quite distinct, viz., the adventitious cambium which forms the core, and the normal cambium of the stem. If the first-named cambium dies, then there is an end of burr formation at that point.

The production of burrs is not a universal habit of *Hevea brasiliensis;* indeed, they are comparatively rare on untapped trees. It has previously been pointed out (*Report of the Mycologist for* 1906) that freedom from burrs is a character which should be required in the selection of seed bearers, there being some evidence that some races of *Hevea* are less liable to produce them than others.

There is nothing to support the statement that these burrs 'work out' if left alone. As already described, the core is not attached to the main wood at the beginning, but unites with it after a few months' growth. Nor is it advisable to knock them out with a stick or kick them off. Either practice tends to split the inner bark from the main stem.

Though these burrs certainly do occur on trees which have never been tapped, they are most often found on renewed surfaces about two years after tapping. The illustration (Plate 9) shows one of the Henaratgoda trees which was tapped with knife and pricker in 1906. Three-quarters of the renewed surface is thickly covered with large burrs, which project three or four inches from the trunk. These are now united to the main wood, but a fresh crop of smaller burrs with free cores is developing between them. On the left-hand side it will be seen that the burr formation has extended to the untapped bark.

As previously stated, these burrs are not caused by insects or fungi, and to some extent are a normal feature of *Hevea brasiliensis.* But it undoubtedly appears that their production can be stimulated by tapping. Ordinary tapping injuries usually produce the swellings first described, and we must look for some other reason for the production of these growths of wood *in* the cortex. I am strongly of opinion that this is to be found in the use of the pricker.

It has been shown that a wound extending to the wood does not, as a rule, produce these nodules in the cortex, but causes a swelling on the main wood. The fact that the nucleus of each free nodule consisted of a group of dead bark cells suggested that they were formed round minute frag-

A B

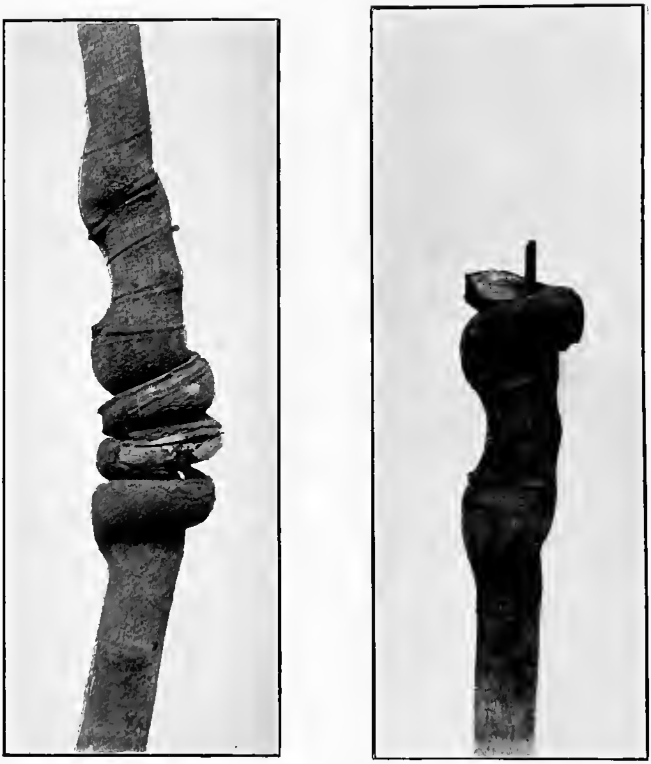

Plate XI.—A Twisted *Hevea* Stem. × $\frac{1}{6}$.

ments of bark which were pushed into the cortex by the teeth of the pricker. Fitting's researches, however, suggest another solution; he has shown that after the pricker has been used the renewing cortex beneath each pricker cut is not laticiferous, but contains an abnormal number of groups of stone cells; it is probable that some of these groups may serve as nuclei for the nodules which occur in pricked cortex.

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A Twisted *Hevea* Stem.

The photographs on Plate 11 show the stem of a two-year-old *Hevea*, two inches in diameter. At a height of six inches from the ground the stem makes three complete turns, and above these it is marked by a spiral groove for a length of nine inches. It will be seen from the photographs that this spiral groove begins near the upper edge of the last coil. The specimen had been broken before it came into my possession, and the fracture is shown by the line across the middle coil, where some of the bark has been broken off in the attempt to fit the two pieces together. The coils are quite free from one another, that is, they are in contact but not fused together. The stem has undoubtedly been coiled completely round three times; it is not merely grooved.

When the stem is broken across the middle turn it is seen to be coiled round a much thinner dead stem. This is evident in the second photograph, which shows the upper part of the stem inverted. From this the explanation of the phenomenon is fairly simple. When the young tree was planted out in the field it was, as usual, 'stumped.' The stem then died back to the next node, and the new leading shoot sprang from the bud at that node. But instead of growing straight up by the side of the dead stem, it coiled round the latter three times. The cause of this coiling is revealed in the second photograph, where, still twined round the dead original stem, is seen part of some climbing weed. This climber grew up the stem of the young plant, and arrived at the bud just as the latter started into growth; and in twining further round the dead part of the stem it carried the young shoot round with it. When the two reached the top of the dead stem the *Hevea* shoot grew straight upwards, and the climber then twisted itself round the green shoot; this is shown by the spiral groove on the upper part of the stem, which is caused by the pressure of the coils of the climber on the young stem as the latter expanded. It is most probable that the coils of the *Hevea* stem were at first wide apart, but that they have come into contact owing to its subsequent thickening. If the tree had been allowed to grow, the coils would no doubt have become fused into a solid mass.

Reference.

Petch.—*Tropical Agriculturist*, Oct. 1909.

Fasciation.

Instances of the fasciation of the stem of *Hevea* are not rare. Most estates can show one example at least, and it has been estimated that one case occurs in about every 10,000 trees. As a rule, the trees affected are about two years old.

In what is perhaps the commonest case, the green shoot gradually expands at the apex and becomes flattened. The edges of this flattened stem increase in thickness more rapidly than the centre, so that it becomes fluted along the middle line, and ultimately it divides into two. Each half now grows independently, and in the course of its growth it curves over towards the centre of the stem and crosses the other. Plate 12, A, illustrates a simple case, in which each half, after crossing the other, expands into a thin plate with a variously scalloped margin, but in many cases each makes several complete turns before expanding.

The wing-like expansions are covered with small projecting ridges, usually arranged in lines running across them; these are the scars of aborted leaves. The edge of the wing is also covered with aborted leaves or stipules crowded together. Frequently normal leaves are borne in large numbers just below the point where the stem divides, and also along the more regular, thicker edge of the wing. This results in the production of a dense cluster of leaves which frequently hides the fasciated stem completely.

A

B

Plate XII.—Fasciated *Hevea* Stems.

Plate XIII.—A Fasciated Stem.

Plate XIV.—A Fasciated Stem. × $\frac{1}{3}$.

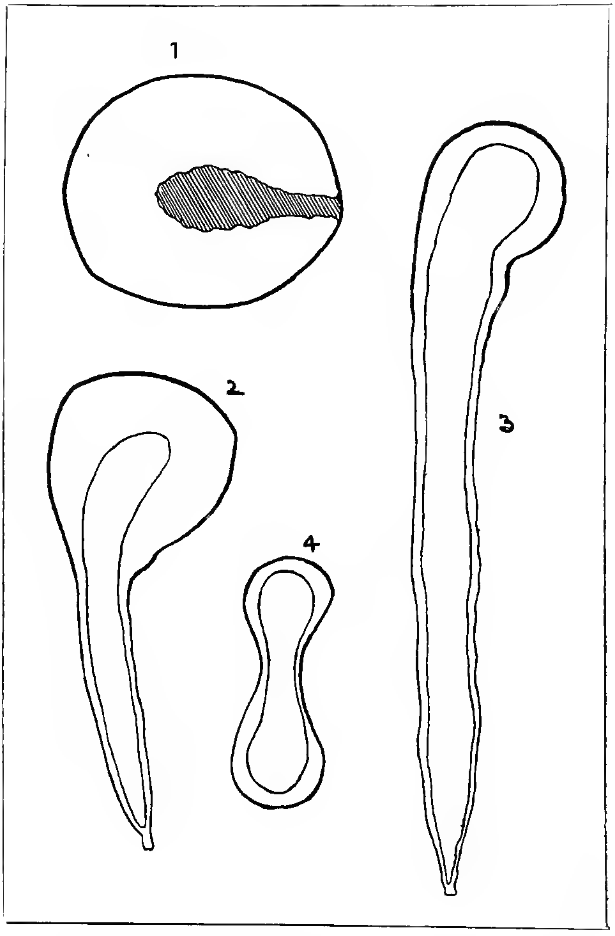

Plate XV.—Cross Sections of Fasciated Stems.

Plate 14 shows a specimen in which one side especially has executed numerous turns, and Plate 13, one of which only one side has developed. Such one-sided specimens are not uncommon, but they are not usually developed to such an extent as the specimen photographed.

A cross section of the stem of the specimen on Plate 12, A, taken just below the point where the stem divides, is shown on Plate 15, fig. 4. The central area consists entirely of pith, and is surrounded by a narrow zone of wood, &c. This zone of wood is thickest at the edges of the stem, and thins away to the constriction along the centre, instead of being the same thickness all round as in normal stems. Higher up, the stem divides into two, and consequently each half has a thicker layer of wood on its outer margin than on the inner. As the halves grow further, each curves from the denser woody side to that where the wood is thinner.

Plate 15 also shows three cross sections through the specimen of Plate 13. No. 1 was cut just below the point where the stem bends over; the shaded part is all pith, and the remainder the wood. As before, the stem curves over towards the side where the wood is weakest. No. 2 is cut through the third turn, where the wing has already begun to develop; at this point the structure is practically a stem furnished with a wing. The section shows a fairly thick zone of wood round the greater part of the pith, and a very narrow zone round the expanded wing. Section 3 was taken across the vertical expansion, about the middle of its length; the stem part of the structure has now almost completely disappeared, while the wing is still further developed. All the centre of the section consists of pith, and the surrounding zone of wood is extremely thin.

In every case the expanded wing consists almost entirely of pith, enclosed in a thin covering of wood. As the stem expands, the volume of wood diminishes, while the volume of pith increases. The stem continues to curve, as long as one edge of it possesses more than a certain thickness of wood.

Fasciation in some instances (other than *Hevea*) is due to the attacks of insects or fungi, but in the majority of cases nothing definite can be stated except that they are certainly not due to either of those causes. There is no reason to believe that either insects or fungi are responsible for fasciation in *Hevea*.

The abnormal tops should be cut off. If left alone the fasciated part of the stem dies; but before that happens a new shoot should develop lower down the stem. I have been informed that, in Java, when a fasciated top has been cut off, the shoots subsequently developed have also been fasciated. Nothing of the kind has been reported in Ceylon, but the specimen photographed (Plate, 12, B) probably affords an explanation. In that instance, the apex of the stem was fasciated, and the two shoots which subsequently developed lower down were also fasciated; these two sprang from the main stem about two feet below the apex, so that if the upper part had been cut off a foot below the apex, the new shoots would have been fasciated. Evidently when the new shoots are fasciated the stem was not cut low enough.

The Effect of Slugs.

On Plate 16 the tops of three young Heveas are shown. The centre one might almost be taken for a case of fasciation. Its apex is curiously clubbed, and consists of a number of arrested shoots, instead of a cluster of developing leaves. Lower down the stem the leaves have disappeared, and side shoots are developing, although the stem is still green. The apices of the other two are somewhat similar, but each has developed several side shoots, and in one instance the buds on these side shoots are giving rise to branches. Under normal conditions each specimen would be a straight green stem, growing only at the apex; but in each case the apex has been converted into a cluster of shoots, most of which have not been able to develop further, while on those which have grown for a short distance the same process is being repeated.

It was soon determined that there was no sign of any fungus in the aborted shoots, and that the effect could not be attributed to insects. But fortunately several shoots bore unmistakable evidence that they had been recently visited by slugs, and the latter were subsequently found in abundance round the affected plants.

The slug which causes the damage, *Mariaella dussumerii*, Gray, has for several years been known to ascend the trunks of *Hevea*, especially in wet weather, and to drink the latex as it flows along the tapping cuts. Probably most planters in the low country have seen it feeding in that way; it is brown, mottled with darker dots and streaks, and two to three

Plate XVI.—The Effect of Slugs.

inches in length. In the present case it had climbed up the stem of the young plant and eaten off the terminal bud, presumably in order to obtain the latex. The plant had then put out fresh buds at the apex, all of which were attacked in the same way ; and this bud pruning induced the development of the buds lower down the stem. The damage was serious, as a large number of plants were attacked, and their development completely checked. It is probable that many similar cases which have been attributed to insects are really due to slugs, as the latter feed chiefly at night and thus escape notice. During the daytime they may be found under dead leaves, &c., round the trees, or among rubbish in any neighbouring jungle.

Ewart, in the *Journal of the Department of Agriculture of Victoria*, December 1910, has given the following method of preventing the attacks of slugs. 'The method is, in brief, to add one or two large cups of phenyl to ten or twenty cups of water, and use the mixture to moisten a bucket of sawdust. The sawdust is then spread round the rows of plants to be protected, or around single plants; if the area enclosed is a large one it is also sprinkled on the surface of the soil. The protective action is remarkable. It persists even after a heavy rain if the sawdust is not washed away, and it lasts for a considerable time. During wet weather a stronger solution can be employed, since the phenyl slowly washes out of the sawdust. No injurious action is exercised on the plants nor upon the soil as the sawdust slowly works into it. The effect of depriving the animals of their food is to cause a marked decrease in their numbers, quite apart from any poisonous action. The labour and cost involved is exceedingly small.'

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CHAPTER XIII.

PREPARED RUBBER.

FUNGI ON PREPARED RUBBER.

IN Ceylon the fungi which grow on prepared rubber are the common moulds which may be found flourishing more luxuriantly on leather goods, damp paper, and miscellaneous vegetable substances such as bread, damp tea, copra, &c. Rubber biscuits which are left lying in the laboratory for a long time develop yellow or black patches of *Sterigmatocystis*, or green patches of *Penicillium*, especially if they are lying on the stone floor. But these growths, as far as can be judged without elaborate tests, do not affect the quality of the rubber. It is generally assumed that mouldy rubber must necessarily be inferior, *i.e.*, that it must have been damaged in some way by the growth of the mould on it, but up to the present this remains an assumption. As far as is known, no fungus is able to live at the expense of the caoutchouc; it can only consume the impurities in the prepared rubber. Hence, if the growth of moulds damages the rubber it must be concluded that the quality of the rubber is dependent upon the impurities in it—the more impurities the better the rubber!—which is improbable, or that the caoutchouc is affected by the substances produced by the fungus during its growth.

At the Ceylon Rubber Exhibition of 1906, samples of rubber from different countries were presented by several firms of London brokers, and these were afterwards placed in the museum at Peradeniya. The cases in which they were exhibited were opened from time to time during the Exhibition, and therefore all the samples were liable to infection by the common Ceylon moulds. These specimens were subsequently examined and compared, as far as their mouldiness was concerned, with (4) samples which had been exposed during the Exhibition and afterwards to the end of the year, and (5) samples exposed during the Exhibition and subsequently enclosed in a museum case. Two possible sources of error may be pointed out; the Ceylon rubber is the more recently manufactured and therefore the more liable to become mouldy, and the foreign rubbers may have passed through a mouldy period before their arrival in England. The first is a point in favour of the foreign rubbers; but the second is most probably invalid, because the samples were in most cases only sections of larger lumps, which were cut in England, and therefore presented a surface not previously subjected to the action of moulds. The mould was the same in all cases, and was a Ceylon species, not an English one.

In the following tables 'irregular lumps' indicates that the rubber was composed of small lumps welded into large masses with numerous interspaces.

CASE I.—SOUTH AND CENTRAL AMERICAN RUBBER.

Kind of rubber.	Nov. 12, 1906.	Dec. 29, 1906.	Appearance.
Hardcure fine Para...	Slightly mouldy.	Very mouldy.	—
Para negroheads ...	Traces.	Mouldy.	Irregular lumps containing earth.
Manaos scrap ...	—	Traces.	Do. do.
Peruvian and upper Amazon ball.	—	Traces.	Irregular sheet containing much bark.
Peruvian slab ...	—	Slightly mouldy.	Large spongy lumps.
Matto Grosso virgin	Traces.	Slightly mouldy.	Large, partly homogeneous lump.
Matto Grosso negrohead.	—	Traces.	Large, irregular lumps; bark.
Manicoba plantation sheet.	Very badly mouldy.		—
Manicoba scrap ...	—	Slightly.	Irregularly wound sheet; contained much bark.
Assare scrap ...	Mouldy.	Very mouldy.	Contained much bark and earth.
Santos Mangabeira...	—	Traces.	Homogeneous lumps.
Nicaraguan scrap ...	Traces.	Mouldy.	Compressed, very barky scrap.
Carthagena scrap ...	—	—	Do. do.
Columbia virgin scrap	—	—	Compressed barky sheet.
Mexican plantation Castilloa.	—	—	Biscuits.

The Manicoba Plantation sheet was the first to become mouldy, and was much the worst; the Mexican Plantation Castilloa had been mouldy prior to its arrival in Ceylon, but the fungus had been cleaned off. The plantation rubbers and the Hard Para developed the most mould, only the Assare scrap approaching them in that respect.

CASE II.—AFRICAN RUBBER.

Kind of rubber.	Nov. 12, 1906.	Dec. 29, 1906.	Appearance.
Red Massai Niggers	—	Traces.	Irregular lumps with some bark.
Gambia Niggers ...	—	Traces.	Do. do.
Congo red Kasai ...	—	Traces.	Do. do.
Congo Lac Leopold III.	—	Mouldy.	Do. do.
Upper Congo Ball ...	—	Traces.	Contained much bark.
W.C. African Lump	Tacky and mouldy.	—	Black homogeneous slabs.
Brown Niger niggers	—	Traces.	More than half bark.
Loanda niggers ...	—	Slightly tacky.	Do. do.
Uganda Plantation sheet.	—	Traces.	—
Uganda Pears ...	Slightly mouldy.	Mouldy.	—
Mozambique ball ...	—	Traces.	Threads in small balls.
Mozambique sausage	—	—	Threads in spindles.
Mozambique unripe ball.	—	Slightly tacky.	Very barky.
Mozambique Lamu ball.	—	—	—
Nyassa ball	Slightly tacky.	—	Contained bark.
Madagascar pinky ...	—	—	—
Tamatave	—	—	—
Madagascar Majunga	—	Tacky.	Contained bark.
Madagascar earthy niggers.	—	Traces.	Threads with a large quantity of earth.

Compared with the American samples, the above were remarkably free from mould, though the majority contained a large proportion of bark or earth. The plantation rubber of this series showed scarcely any mould, but native rubber from the same country became mouldy.

CASE III.—EAST INDIAN RUBBER.

Kind of rubber.	Nov. 12, 1906.	Dec. 29, 1906.	Remarks.
Plantation Assam ...	—	Slightly mouldy.	Compressed scrap, containing bark.
Red Assam	—	Traces slightly tacky.	Contained bark.
White Assam ...	—	Very tacky.	Do. do.
Red Rangoon ...	—	—	Threads in balls with much bark.
Red Penang... ...	—	—	(Threads in balls with much bark.)
White Penang ...	—	Tacky.	Contained much bark.
Borneo	—	—	—
Tonquin strips ...	—	Slightly mouldy.	—
Palembang	—	—	—

Very little mould developed on these specimens, but a larger proportion were tacky than in the other cases.

IV.—CEYLON RUBBER, EXPOSED SEPT. 13TH TO DEC. 29TH, 1906.

Biscuits	The top biscuit of a pile was slightly mouldy, while the others were mouldy on the exposed edges.
Unwashed scrap ...	Very slightly mouldy.
Washed scrap ...	Traces.
Crepe	Not mouldy.
Ceylon blocked crepe...	Slightly mouldy.

V.—RUBBER, IN MUSEUM CASE, DEC. 29TH, 1906.

Hard Para	Dense patches of mould on the cut surface. The specimen was half a lump.
Lanadron block ...	Fairly mouldy, but not as bad as the Para.
Crepe	Not mouldy.
Biscuits	Made by Parkin in 1899; no signs of mould.

The first point of interest is the susceptibility to mould of almost all the plantation rubbers. Manicoba Plantation sheet was green with mould within a few weeks. Mexican (biscuits) had evidently been mouldy previously and did not develop any more. Assam plantation rubber turned only slightly mouldy, but this is a form which is more comparable with Ceylon scrap than Ceylon biscuits. The outstanding plantation rubber was Uganda sheet, which showed scarcely any trace of mould. Ceylon biscuits turned slightly mouldy wherever exposed, but washed scrap was practically free, while crepe was quite free from mould. This was very striking in the case of the crepe of Table V., which was laid on the top of the Lanadron blocks; the latter were covered with scattered patches of mould, but the crepe was free from it. The mouldiness of the hard cure Para appears to throw doubt on the efficacy of smoking or creosote as a preventative of moulds, but, on the other hand, Parkin's creosoted biscuits, made in 1899, which were lying next to the hard cure Para, showed no signs of mould.

The most striking feature of the series was the comparative immunity of nearly all the wild rubbers. This is quite contrary to our *à priori* theories. It would have been expected that the wild rubbers, naturally coagulated on the tree, or collected on the ground, and mixed with large quantities of bark and earth, would have developed more mould than the more carefully prepared plantation product; yet the wild rubbers, with hardly any exception, showed only the slightest traces. They might be sticky or tacky, but they were not mouldy. In spite of obvious objections, it is apparently a fair conclusion that the wild rubbers are not so susceptible to mould as the plantation forms and the hard cure Para. It might be suggested that the use of acids in coagulation favours the development of fungi, since fungi, as a rule, grow best in an acid medium, but this would scarcely affect the growth of the species concerned in this instance. The moisture in hard Para would certainly assist in promoting the growth of mould. The difference between crepe and biscuits may depend upon the fact that the former is more thoroughly washed and more quickly dried.

A pile of dry Ceylon biscuits develops mould on the top biscuit and on the exposed edges, but there is none between the biscuits. If, however, the biscuits are piled on one another before they are dry, a thick felt of mould is developed between them. Any damp rubber will, unless treated with a strong fungicide, be certain to turn mouldy in a climate like that of Ceylon, and to avoid moulds it should be dried as rapidly as possible. The common mould on rubber in Ceylon is *Eurotium candidum*, Speg.

Red Patches.

Red patches frequently appear on prepared rubber, usually when it is drying. They are commonest on biscuits, but often occur also on crepe. Probably they are not so often noticed on crepe, because the latter is frequently a dark colour. They vary in size up to half an inch or so in diameter, but are not necessarily circular. The colour is a clear red, and extends right through the biscuit. They are not visible on the fresh biscuit, but appear when it is about half dry; and they gradually fade if the rubber is kept for some time.

Carruthers stated that they were caused by the growth of a fungus, a species of *Syncephalis*, on the rubber. Ridley has attributed them to the action of *Protococcus nivalis*, the alga which colours the snow red in the Alps. Brooks (Sarawak) considers that they are due to *Bacillus prodigiosus*, the bacillus which sometimes produces red spots in bread; he states (*Agric. Bull. Straits*, January 1911) that inoculations from freshly visible spots were made by him on sterilised bread and agar agar, and a strong crimson culture was obtained in a few days.

An examination of these red spots in Ceylon has not revealed the presence of any organism to which they could be attributed. It is quite certain that they are not caused by a fungus or an alga; and, though they are such as would be expected to be produced by bacteria, the presence of the latter has not yet been demonstrated.

Brooks states that the colour, in crepe, is almost completely removed by prolonged soaking in methylated spirit. The colouring produced by *Bacillus prodigiosus* is soluble in alcohol, but one would not expect it to be extracted from a rubber biscuit of ordinary thickness.

Black Spots.

In one instance black spots appeared upon the white wet biscuits a day or two after they were made. The spots were all circular, as is usual with colonies of bacteria, and varied in size from minute points to patches an inch in diameter. The discoloration extended right through the biscuit, and, when they were dry, numerous dense black points were visible in each patch, in addition to the general black colour. These patches contained numbers of bacteria, together with minute particles of a black pigment to which the discoloration was due. There is little doubt that the colouring matter was produced by the bacteria. In this case formalin had been used in the coagulation.

Mottled Biscuits.

In many examples of dark-coloured biscuits it will be found that the colour is chiefly due to a thin film of brown colouring matter on the upper side of the biscuit. It is true that the whole of the rubber turns dark, to some degree, but the effect is in most cases increased by the presence of this film of some substance other than rubber. If the biscuit is sliced into two horizontally, a marked difference in colour between the two halves will be evident; one of them is more or less amber-coloured, while the other, though of the same thickness, is dark, owing to the presence of this brown film. The presence of this film may be more clearly demonstrated by cutting a thin slice through the biscuit and placing it in chloroform or some other solvent under the microscope; as the rubber absorbs the solvent and swells, the film on one side shows up quite plainly. This film is composed of some brown amorphous substance whose nature has not been ascertained.

As a rule the film is spread uniformly over one side of the biscuit. In one instance, where it seemed to be more highly developed than usual, it was collected into patches, so that the biscuits appeared rough on one side, and mottled, light and dark, when held up to the light. On dissolving out the rubber, yeast cells were found in abundance among the amorphous brown sediment.

A dark biscuit was sliced into two horizontally, and separate analyses were made of the dark upper portion and the paler lower half. In addition, analyses were made of a dark rough biscuit, mottled as described above, and a pale, amber-coloured biscuit made at the same time on the same estate. The results are given below; it may be noted that the analyst was not aware of the sources of the different samples.

	Dark biscuit.		Biscuits from same estate.	
	Pale side.	Dark side.	Pale biscuit.	Mottled and rough.
Moisture	0·50	0·50	1·00	1·00
Ash	0·25	0·30	0·30	0·20
Resin	1·20	1·40	1·90	1·70
Proteids	3·50	3·68	3·87	3·31
Caoutchouc ...	94·55	94·12	92·93	93·79

These results do not throw any light upon the nature of the brown film. In the analyses of the two halves of the same biscuit, that of the upper dark side shows an excess of 20 per cent. in the ash, 16 per cent. in the resin, and 5 per cent. in the proteid; but in the analyses of the light and dark biscuits the differences are all in the other direction. It is probable that the methods of analysis employed in the examination of rubber at the present time are not sufficiently detailed to determine all its constituents; it is not enough to lump everything other than caoutchouc as resin and proteid.

Bacteria and yeasts appear to be the chief organisms concerned in this spotting of rubber biscuits; of course, moulds grow on the surface, but I have not yet observed any effect which could be attributed to them. An exact investigation into the causes of these spots is required, but it would demand more time than the subject deserves, and more appliances than are, in general, at the disposal of workers in the tropics. It would require a strictly scientific examination, in each case, of the fungus and bacterial flora of the collecting cups, the setting pans, the curing house, and the water supply, together with experiments to determine which of the organisms found would grow in latex or wet rubber, and their effect on either. But although this problem cannot be dealt with under the present circumstances, it is possible to lay down more or less general empirical rules as to the course to be adopted in order to get rid of the cause of these brown or black spots. It is unlikely that the effect is in any way connected with the tree, and it must be assumed for the present that some organism is introduced into the latex or the coagulated rubber either by the wind or by the water supply. In either case, if the collecting cups, pails, &c., are once infected, they will remain infected, and the biscuits will continue to be discoloured, until some method of sterilisation is adopted. Therefore, when this trouble makes its appearance, all collecting cups should be boiled, and the dishes, pails, &c., scalded with boiling water. It has been found sufficient to do this once, but it would be a wise precaution to scald the dishes and pails periodically as part of the general routine of the factory. In one instance, that of the mottled biscuits referred to above, this treatment was adopted; and the superintendent writes: 'With reference to the black biscuits about which I wrote to you some months back, it may interest you to know that since I took your advice and boiled all the utensils used, and had my store thoroughly cleansed, no black biscuits have put in an appearance.'

If the infection is introduced with the water supply, the above treatment will not stop it, because the dishes will be reinfected. To determine whether the water supply is at fault, biscuits should be made, using water which has been boiled and cooled, and these should be compared with biscuits made with the unboiled water. Of course, the dishes, &c., must be sterilised before the experiment is attempted, otherwise that possible source of infection will not be excluded. If the water were infected, and no other source of supply were available, more elaborate experiments would be required to determine whether the infection could be avoided.

Collecting Cups.

Tin cups have been largely used for collecting the latex. It has been objected that as they corrode on exposure they are difficult to keep clean, and it has further been stated that they are objectionable because the plant acids act upon the metal and introduce traces of iron into the manufactured rubber. The last objection might just as well be urged against washing machines; in any case proof should be required that any perceptible amount of iron is so introduced. Tin cups have the advantage that they can readily be sterilised without injury, merely by boiling them.

Paper cups have been tried, and found unserviceable. Those tried in Ceylon were made in two parts, and soon came to pieces. Enamelled iron cups have been used to some extent, but as a rule the enamel soon chips, and they are then no better than ordinary tin. Glass cups have found more favour in the Federated Malay States than in Ceylon, owing to the absence of stones on the rubber estates in the former country; they can be kept very clean, but cannot be sterilised by scalding, though they might withstand boiling; the cost of transport to the East is the chief objection against them.

Coconut shells were used in the early days of rubber tapping, and it is to be regretted that they are now being re-introduced on the ground of cheapness. It is impossible to keep them clean for any length of time; the inside turns black and decays, and though they may be scraped, that process is practically useless. It would require prolonged boiling to sterilise them, and they would be more liable to decay after sterilisation than before. Their use could only be advised if it were proved that no fungi, nor bacteria, nor any products of decomposition had the smallest effect upon the caoutchouc. At the present time, when any deterioration of rubber is attributed to the action of bacteria, it is surprising that the planter should have reverted to coconut shells.

Tacky Rubber.

This defect is unfortunately too well known. The rubber turns sticky on the surface, and this condition spreads through it until it is converted into a viscid mass resembling birdlime. Frequently the colour changes at the same time to a rather pale yellow. It is said that all kinds of rubber are liable to become tacky, with the exception of hard Para, and from this it has been argued that the ability to undergo this change depends upon the botanical origin of the rubber. But since Eastern Plantation Hevea, which has presumably the same botanical origin as hard Para, becomes tacky, this contention would appear to be unsound.

Experiments to determine the cause of tackiness have, in general, been carried out only in temperate climates; but an exception to the general rule is afforded by Bamber's experiments in Ceylon. Bamber has stated he was able to cause rubber to become tacky by inoculating with slight traces of tacky rubber, and that in conjuction with a colleague he had found bacteria, fungi, and an oxidising enzyme in the tacky rubber. He affirmed that this condition may be caused by bacteria or fungi, with probably the production of an oxidising enzyme. Detailed accounts of these experiments have not been published.

Experiments at Peradeniya have not confirmed the above. Tacky rubber may contain bacteria, but it does not as a rule; and fungi are seldom found on it. On the other hand, many cases of bacteria in rubber have been observed in which there was no indication of tackiness. Nor has it been found possible to communicate the 'disease' by transferring the tacky rubber to apparently sound pieces. In one case, a piece of crepe rubber, partly tacky, was selected, and some of the liquid mass was smeared over the sound part; the whole was covered with a bell glass to prevent further drying. After two years, there was no difference in the condition of the sound rubber. In another case, part of a sample of rubber from old trees was placed near a window where the sun's rays fell on it, and it became tacky. Some of this was taken, and placed on a sound biscuit from the same lot, and covered with a bell glass. Again there was no communication of the tacky condition to the lower biscuit, though it was left for two years. In the latter instance the whole of the rubber had been coagulated in the same way; it all came from the same trees; and it had never been removed from the room in which it was coagulated.

The only known way of causing raw rubber to become tacky is to expose it to sunlight or heat. That at once throws doubt upon the theory that the cause is to be found in the action of bacteria. For it is well known that sunlight is inimical to bacteria and that the majority are killed by exposure to it. Yet rubber exposed to sunlight becomes tacky, it is said, in a few hours—a much shorter time than would be required for any marked development of bacteria.

Again, Professor Bertrand has pointed out that it cannot be imagined that any bacterium can feed on the actual caoutchouc. It may live upon the proteids, &c., in the rubber, but not upon the rubber itself. H. C. T. Gardner has stated that even the use of corrosive sublimate as a coagulant does not prevent tackiness—that rubber thus prepared from *Funtumia* latex is prone to undergo comparatively rapid deterioration; this again is opposed to the bacterial theory, and in the face of such a result it would seem impossible to uphold the view that immunity of hard Para is due to the fact that it is cured with smoke. Vulcanised rubber becomes tacky, as well as raw rubber.

Gardner has put forward the theory that the initial cause of tackiness is 'nothing more than moisture.' As he states, this theory has the advantage of simplicity, but it also has the disadvantage of disagreeing with known facts. Plantation Hevea contains less moisture than other kinds; it should therefore be less liable to become tacky, if that theory is correct. It is to be noted that the rubber does not become tacky during the process of drying, but some time after it has completely dried, even after it has been packed and sent to Europe. Moreover, this theory would scarcely explain why if one of two biscuits from the same batch is placed in full sunshine while the other is kept in the shade, the former becomes tacky; surely in the tropical sunshine it should lose moisture and become drier than the other biscuit.

The following analyses of sound and tacky Hevea rubber have been published by Bamber.

	Sound rubber.	Tacky No. 1.	Tacky No. 2.	Very Tacky.
Moisture	0·30	0·36	0·06	0·44
Ash	0·38	0·28	0·54	0·72
Resin	2·36	2·32	2·66	3·70
Proteids	3·50	3·85	3·50	4·90
Rubber	93·46	93·19	93·24	90·24

These throw no light upon the subject. The samples are presumably from different sources, and the amount of variation in the different constituents is not greater than would be expected under such circumstances, except perhaps in the case of the proteid in the last sample; but one can scarcely argue that the rubber has been converted into proteid.

Spence has been for some time engaged on the investigation of tackiness, and has published an account of some of his experiments. He has pointed out that none of the various theories which have been propounded in explanation of this phenomenon, *e.g.*, oxidation, putrefaction, the action of bacteria, or enzymes, have any scientific basis, and agrees with Bertrand that bacteria cannot be the direct cause of tackiness.

Spence has described an experiment in the coagulation of *Funtumia* latex which had been preserved in a liquid state by means of ammonia. The ammonia and salts were separated by dialysis, and the latex was then sterilised. Part of the sterilised latex was coagulated by means of decinormal sulphuric acid, while the other was treated with sterilised water only. Absolute alcohol was then added to both, and they were heated to 100° C. to obtain complete coagulation in both cases. The rubber obtained by water and alcohol only was a white elastic mass with very good tenacity and with 'nerve,' while the rubber coagulated with sulphuric acid was very soft and plastic without either tenacity or 'nerve.' After the samples had been washed and dried, the former was a sample of good rubber, while the latter melted into a soft resin-like paste. Thus from the same sample of latex, two entirely different specimens of rubber were obtained, the one sound and the other excessively 'tacky.' The dry weight of the samples proved that no 'oxidation' had taken place, and acetone extraction showed that the percentage of resin was practically the same in both, while further analysis proved that the tackiness was not due to chemical changes. These facts, together with experiments on the viscosity, &c., lead the author to the conclusion that tackiness depends not on chemical changes but on physical changes.

Practical observations on tackiness are all, at present, more or less vague. It is confidently asserted that tackiness is communicable, *i.e.*, that a tacky biscuit infects others in contact with it, but the possibility that, in the supposed instances of this, similar external or internal conditions may have produced tackiness in these biscuits successively is overlooked. It is not sufficient to put a tacky biscuit on the top of an apparently sound one, and then to argue that the 'disease' has been transferred when the lower biscuit becomes tacky. It is said that the rubber obtained from the first tappings of any tree is more liable to become tacky than that yielded subsequently, but no evidence has been offered in support.

CHAPTER XIV.

OTHER FUNGI ON *HEVEA*.

IN addition to the fungi already referred to in the previous chapters, the following species, most of them only saprophytic, have been recorded on *Hevea brasiliensis*. Any mycologist who cared to devote the time to examining dead leaves, stems, &c., of *Hevea* would be able to extend this list tenfold, but no useful purpose would be served by doing so, because the majority of the fungi found on dead *Hevea* are quite harmless.

Ascomycetæ.

Asterina tenuissima, Petch.—Extremely thin, forming a blackish discoloration on branches and fruits of *Hevea*, spreading indefinitely. Mycelial hyphæ brown, 4–5 μ diameter, smooth, united when old by a film of mucus, bearing numerous septate, erect hyphæ, 90–100 μ long, olivaceous, with acute tips; perithecia flattened, black, 130–160 μ diameter, ostiolate; asci, 30–40 × 9–12 μ, clavate; sporidia, 13 × 4 μ, one-septate, constricted, fusoid, hyaline.

On green stems and fruits of *Hevea brasiliensis*.

This species is not parasitic on the *Hevea*, but lives on the sugary secretions of the nectaries at the base of the leaf. The black discoloration occurs on practically all green *Hevea* stems, but it is probable that it is caused by different species of 'sooty moulds' in different localities. In addition to this extremely thin black covering, *Hevea* stems and leaves may acquire a much thicker and looser coat of 'sooty mould'; this thicker covering, which usually scales off when old, lives upon the secretions of scale insects, and if it is desired to remove it, the trees must be sprayed with an insecticide to get rid of the insects. As a rule, the sooty mould which lives on the secretions of the nectaries is inconspicuous; a conspicuous covering is due to sooty mould following insects.

Nummularia pithodes (B. & Br.), Petch.—Widely effused, up to 30 cms. long and 15 cms. broad, or confluent for a length of one or two metres, on erect or fallen tree trunks; developing in the cortex and forcing off the outer layers; adnate, flattened, 2–3 mm. thick, distinctly margined by the cortex, carbonaceous, black, dull or sometimes shining, here and there slightly undulating but usually plane, smooth or minutely papillate. Perithecia densely crowded, vertically elongated, hexagonal in section, 0·5 mm. diameter, 1·5 mm. high; ostiola usually not projecting. Spores dark brown, broadly cymbiform, or fusoid-elliptic, inequilateral, 25–35 × 9–11 μ. Asci not measured.

This species was found in Ceylon by Thwaites in 1867, and was sent by him to Berkeley and Broome, who named it *Diatrype pithodes*. It is not uncommon in up-country jungles in Ceylon, where it forms large black patches, like patches of asphalt, on dead tree trunks. Recently it has been found on dead *Hevea brasiliensis* at Singapore, and has been re-described by Massee as *Eutypa caulivora*. But it is an old, well-known species, and there is as yet no reason to believe that it is other than saprophytic.

Diaporthe heveæ, Petch. — Perithecia distinct, black, 0·5–1 mm. diameter, in small groups, embedded in the wood; ostiolum about 0·1 mm. diameter, up to 0·5 mm. long, projecting slightly above the surface of the bark; asci, 40–45 × 6–8 μ, narrow oval, eight-spored; sporidia obliquely uniseriate, hyaline, fusoid, one-septate, 10–13 × 4 μ.

In branches of *Hevea brasiliensis*, Ceylon, apparently parasitic, but found only on one occasion.

Parodiella melioloides (B. & C.), Wint.—Perithecia globose, imperforate, minute, 0·16–0·2 mm. diameter, covered with a red pruina, situated in a branched, septate, blackish-red mycelium; asci large, inflated in the middle, gradually rounded above, thick-walled, shortly stalked, 120–130 × 35–45 μ, eight-spored; paraphyses absent; sporidia, oblong, rounded at the apex, one-septate, slightly constricted at the septum, the upper loculus the longer, 40–42 × 12–15 μ, for some time hyaline or pale yellow, granular, finally yellow brown.

On leaves of some species of *Hevea*, but whether *Hevea brasiliensis* or not is not stated; Brazil.

Ophiobolus heveæ, P. Henn.—Spots, gray, rounded or confluent; perithecia epiphyllous, scattered, immersed, ovoid, about 250 μ diameter, black, submembranaceous; ostiola subconical, obtuse, somewhat shining, erumpent; asci subfusoid or clavate, somewhat thickened at the apex, rather obtuse, 60–70 × 7–10 μ, eight-spored; paraphyses filiform, hyaline, about 2 μ thick; sporidia parallel, filiform, ends obtuse, hyaline, pluriguttulate, finally subseptate, 50–60 × 2–3 μ.

On the leaves of some species of *Hevea*, but whether *Hevea brasiliensis* is not stated; Brazil.

Nectria coffeicola, Zimm. — Perithecia crowded, stroma wanting, sessile, globose, 0·3 mm. diameter, 0·4 mm. high, at first red, then brownish; ostiolum slightly elevated, colourless or finally bluish; asci cylindrico-clavate, eight-spored, 70 μ long; sporidia ellipsoidal, one-septate, not constricted, ends obtuse, 10–13 × 5–6.

Originally found on dead stems of *Coffea arabica* in Java: afterwards on dead branches of *Hevea brasiliensis* in company with *Corticium salmonicolor*.

Nectria diversispora, Petch. — Perithecia 0·25 mm. diameter, solitary or in small groups, without any stroma, red, translucent, rough with minute papillæ; ostiolum conical, ochraceous; asci 80–100 × 10–15 μ; spores obliquely uniseriate, red-brown in mass, 11–13 × 4–5 μ, one-septate, constricted at the septum, oval, ends obtuse, wall striate, one cell often larger than the other, and one or both ends often rounded in a circular arc.

On dead bark and dead fruits of *Hevea brasiliensis*, Ceylon. This is the common red *Nectria* on dead *Hevea*, and the species to which canker and pod disease were formerly attributed. It is, however, merely saprophytic. It is probable that this species is identical with the foregoing, in spite of the differences between the descriptions.

Calonectria cremea, Zimm.—Perithecia superficial, generally crowded, globose, pale, creamy yellow, 0·2 mm. diameter; ostiolum slightly elevated, colourless; asci clavate, with a short, thick, nodulose pedicel, four-spored, no paraphyses; sporidia rounded-oblong, ends rounded, slightly curved, four-celled, slightly constricted at the septa, hyaline, 23–25 × 7–8.

First found on dead fruits of cacao in Java: common on dead stems of cacao and *Hevea brasiliensis* in Ceylon. Probably identical with *Calonectria flavida*, Mass. Saprophytic.

Megalonectria pseudotrichia (Schw.), Speg.—Perithecia globose, at length collapsed and apotheciiform, red or brick-red, glabrous, 350 μ diameter, usually crowded; asci clavate, 70–75 × 25, eight-spored; sporidia elliptical, ends obtuse, 5–7-septate, muriform, constricted at the median septum, 25–30 × 9–11 μ.

Conidial stage (*Stilbum cinnabarinum*, Mont.) 2–4 mm. high, 0·3–0·4 mm. diameter, erect, red; conidia, eliptical or ovoid, 5–6 × 2.

This species is common on dead cacao or dead *Hevea* stems. It resembles a red nectria, but is distinguished by the numerous minute stalks, like small red pins, which grow with the nectria perithecia. It is in general purely saprophytic. I have noted one case in which it appeared to be parasitic, but in the light of subsequent experience that record must be considered doubtful.

Phyllachora Huberi, P. Henn.—Stromata hypophyllous, on yellowish spots, rounded or irregularly flattened, thin, crustaceous, black, opaque, 3–11 mm. diameter, sometimes confluent, ostiola wide; perithecia gregarious, immersed, subglobose, pallid within; asci clavate, rounded at the apex, attenuated at the base, 8-spored, 50–65 × 16–20 μ; sporidia distichous or obliquely monostichous, ovoid or subfusoid, hyaline, granular within, 14–18 × 8–10 μ. On leaves of young *Hevea brasiliensis*. Para.

Dothidella Ulei, P. Henn.—Stromata cæspitose, erumpent, ovoid, black, rugulose, about ⅓–3 mm. diameter; perithecia few, ovoid, immersed; asci clavate, rotundato-obtuse, 8-spored, 50–80 × 10–16 μ; paraphyses present; sporidia subdistichous, oblongo-clavate, hyaline, one-septate, scarcely constricted, 13–20 μ. In leaves of *Hevea brasiliensis*. Rio Jurua.

Tryblidiella Leprieurii (Mont.), Sacc.—Perithecia sessile, boat-shaped, or triquetrous, clustered; disc reddish, fleshy, dotted with black sporidia, when wet scarcely a line wide, when dry closed by thick, incurved, brownish-black lips; asci clavate, 180 μ long, eight-spored; sporidia, straight or curved, oblong, triseptate, fuscous, 32–40 × 10–12.

On dead branches of *Hevea*; Ceylon. Saprophytic.

Sphærioidaceæ.

Phyllosticta heveæ, Zimm.—On brown spots at the ends of the leaves; pycnidia on the upper surface, crowded, covered by the epidermis; then erumpent, somewhat flattened, brownish black near the ostiolum, 80–150 × 60; ostiolum 16 μ broad; sporules elliptic, ends acute, hyaline, 6–7 × 2·5, biguttulate.

Found on leaves of *Hevea brasiliensis* in Java; also on leaves of seedling *Hevea* in Ceylon. ? Parasitic.

Phyllosticta ramicola, Petch.—Pycnidia 0·1–0·25 mm. diameter, black, subepidermal, crowded, slightly prominent, lenticular, 75–140 μ high; spores narrow-oval with sharply-pointed ends, 8–12 × 2–3 μ, greenish hyaline, often biguttulate, issuing in a fine white tendril.

On green stems of *Hevea brasiliensis*, in company with *Glœosporium albo-rubrum*, Petch. Ceylon.

Phoma heveæ, Petch.—Pycnidia black, hemispherical, gregarious, immersed, slightly prominent, 0·1–0·2 mm. diameter; spores elliptical, hyaline, 4–5 × 2 μ, extruded in a greenish yellow tendril.

On branches of *Hevea brasiliensis*. Ceylon.

Aposphæria Ulei, P. Henn.—Pycnidia black, carbonaceous, almost spherical or ovoid, erumpent, appearing completely superficial, scattered or crowded, papillate, about 120–160 μ diameter; sporules cylindrical or fusoid, straight or somewhat curved, hyaline, 6–10 × 0·8–1 μ, containing two or three small oil drops.

On leaves of *Hevea brasiliensis*, with *Dothidella Ulei* and *Phyllachora Huberi*. Brazil.

Sphæronema album, Petch.—Pycnidia semi-immersed, spherical, hyaline, 140–260 μ diameter, produced above into a hyaline, longitudinally striate tube, 250–800 μ long, 80–160 μ diameter at the base, 40–80 μ diameter at the apex where it splits into linear spreading teeth; sporules oval, hyaline, continuous, 7–11 × 4 μ.

Saprophytic, on decaying fruits of *Hevea brasiliensis*.

Ciliospora gelatinosa, Zimm.—Pycnidia densely crowded, globose, or angular through mutual pressure; ostiolum conical, slightly prominent; subgelatinous, hyaline, about 1 mm. diameter; wall about 60 μ thick, composed of parallel hyphæ; sporulæ cylindrical, almost straight, continuous, 15–30 × 5–6 μ, furnished with four to eight slender cilia, 7–12 μ long, near the ends.

Common on decaying *Hevea* bark: saprophytic.

Melanconiaceæ.

Glœosporium brunneum, Petch (name only, in Report of the Mycologist, Ceylon, for 1905) = *Glœosporium heveæ*, Petch.

Glæosporium elasticæ, Cooke and Massee.—Pustules minute, scattered, becoming black; sporules ellipsoid or elongated, ends rounded, hyaline, granular, sometimes guttulate, 12–20 × 5 μ.

Originally found on leaves of *Ficus elastica* in Scotland; since discovered on leaves of *Hevea brasiliensis* in Java by Zimmermann, and on leaves of seedling *Hevea* in Ceylon. Koorders (*Bull. Algemeen Proefstation, Salatiga*, No. 3) considers that it is identical with *Colletotrichum ficus*, Koorders.

Colletotrichum heveæ, Petch.—Acervuli black, scattered on the upper surface of the leaf, 0·1–0·25 mm. diameter, surrounded by violet-black, one-or-two-septate, obtuse setæ, up to 90 μ long; sporules oblong with rounded ends, hyaline, granular, 18–24 × 7·5–8 μ; basidia, 20–30 × 6–7 μ, swollen upwards.

On leaves of seedling *Hevea brasiliensis*, Ceylon.

Pestalozzia palmarum, Cooke.—Erumpent, black, gregarious or scattered; acervuli sphæriæform; conidia fusiform, quadriseptate, pale fuscous, with three setæ; pedicel long and hyaline; 15 × 5–6 (parte colorata).

Originally found on coconut leaves; since found on tea, *Hevea*, &c., but generally recorded under the name of *Pestalozzia guepini*, Desm.

Hyphomycetæ.

Periconia pycnospora, Fres.—Conidiophores erect, rigid, brown or fuliginous, simple, 200–300 × 10–14, paler above, two or three septate; conidia sessile, brown, rough, 12–17 μ diameter, borne at the apex of the conidiophore.

Found on diseased leaves of *Hevea* in Ceylon. Saprophytic.

Allescheriella uredinoides, P. Henn.—Pustules pulvinate, rounded or confluent, widely effused, cinnamon or pale ochraceous, subvelutinate; hyphæ slender, branched, septate, sometimes inflated hyaline or slightly yellowish, up to 20 μ thick, conidiferous branches 4–6 μ thick; conidia subglobose, ovoid, pyriform, or oblong, brownish orange, guttulate, smooth, 12–25 × 10–19 μ.

On dead stems of some species of *Hevea*; Brazil.

Cercospora sp.—Specimens of the common leaf disease of *Hevea* in the Federated Malay States were forwarded to Kew, and in the absence of any spores by which it might be identified, it was suggested that it might be due to a *Cercospora* (*Agric. Bull. Straits*, vol. iv., p. 271). It is most probable that it was *Helminthosporium heveæ*.

Ceratosporium productum, Petch.—Creeping hyphæ olivaceous, shining, 4 μ diameter, bearing spores in groups of two to four; spores 9–13-septate, not constricted, olivaceous, becoming paler towards the tip, 130–200 μ long, 10–12 μ diameter at the base, 5 μ diameter at the apex; loculi cubical at the base, increasing in length towards the apex.

On dead branches of *Hevea brasiliensis*, Ceylon.

Stilbum heveæ, Zimm.—Synnemata separate or fasciculate, scattered, about 1 mm. high; stalk thickened at the base, round, red, pink above, 0·5–0·8 mm. high, clothed with curly, hyaline hairs, 10–20 × 4–5 μ; head globose, waxy, rose-coloured, about 250 μ diameter; conidia ovoid, hyaline, continuous, 4–6 × 2–2·5 μ.

On branches of *Hevea brasiliensis*, Java; probably not parasitic.

Basidiomycetæ.

Pleurotus angustatus, B. & Br. (=*Pleurotus flabellatus*, B. & Br.).—Cæspitose; pileus 1–3 inches across, soft and tender, eccentric, fan-shaped, smooth or slightly tomentose, white or bluish, often with a pink tinge; stalk cylindric, white, reticulate, often connate, tomentose; gills white, narrow, decurrent; spores oblong, 7–10 μ long.

A common saprophyte on fallen logs of all kinds. It has occurred on decayed tapping surfaces, where its mycelium penetrated to the sound wood in the form of thin reddish plates.

Marasmius rotalis, B. & Br. — Pileus hemispherical, umbilicate, about 3·5 mm. diameter, with 6–12 deep grooves, with sometimes a minute umbo at the base of the umbilicus, yellow brown; stalk black, hair-like, shining, up to 1 inch high.

The mycelium of this fungus is thin, black, and shining, and resembles a horsehair; hence it is known as 'Horsehair Blight.' It creeps over the branches and stems of trees and shrubs, especially in damp situations where the plants are heavily shaded, fastening itself to the plant at intervals and forming dense tangles. Sometimes, on *Hevea* among tea, it spreads from the tea to the base of the *Hevea* stem; but it is quite superficial and does no damage.

Polystictus persoonii, Fr.—Pileus corky, flattened, rugulose, obsoletely zoned, glabrous, purple red behind, with a broad white margin; lower surface at first porous, then labyrinthiform, white.

A common, saprophytic 'bracket fungus,' which frequently grows on dead *Hevea*.

Poria vincta, Berk.—Resupinate, rather thick in the centre, thin and partly free at the margin, reddish above; pores small, pallid, substance wood colour.

Found in Ceylon on one occasion at the collar of a tree killed by root disease, which was said to have just died. It has not been found a second time, and was probably only saprophytic.

Hexagonia discopoda, Pat. & Har.—Pileus leathery, thin, circular or reniform, polished and gray or grayish brown on the upper surface, zoned with regularly concentric, crowded, darker furrows. pores on the lower surface hexagonal, gray.

A common species on dead branches: generally regarded as saprophytic, but as it is practically confined to dead branches still attached to the tree, there is some ground for supposing that it is parasitic. It is readily distinguished by its rather wide hexagonal pores. Previously recorded for Ceylon as *Hexagonia polygramma*, Mont.

Lopharia mirabilis (B. & Br.), Pat.—White, orbicular, determinate, often confluent; margin usually incurved, and tomentose below; covered with a network of acute-edged ridges and scattered triangular teeth.

Saprophytic, on dead *Hevea* branches; Ceylon.

Hirneola polytricha, Mont.—Stalked or sessile; orbicular, or cup-shaped, or ear-shaped: bluish purple above, clothed with white or brown hairs on the lower surface; horny when dry, subgelatinous when moist.

This a very common fungus on dead *Hevea*, especially on stems killed by *Corticium salmonicolor*. It may be distinguished by its subgelatinous texture. Saprophytic.

Heterochæte tenuicula (Lev.), Pat.—Resupinate, effused; pileus orbicular or elongated, cohering, membranous; circumference white, entire, somewhat free; aculeæ minute, cylindric, equal, scattered, fuscous.

Saprophytic, on dead *Hevea* branches; Ceylon.

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